Representing the user: a sociological study of the discourse of human computer interaction

Thesis

Representing the User

A Sociological Study of the Discourse of Human Computer Interaction

Ph.D. Thesis

Sociology; Human Computer Interaction

September 1991

Geoff Cooper B.A., Dip.Comp.Sci.

The discipline of Human Computer Interaction (HCI) was established in the early 1980's on the foundations of cognitive psychology, computer science and ergonomics. In recent years however, claims have been made for the relevance of forms of sociology to this avowedly multidisciplinary field, and a number of sociologists have attempted to contribute to the general enterprise of developing a deeper understanding of the computer user and thereby informing, and improving, software design. This thesis is, in one sense, a continuation of this emerging body of work. However, my contention is that little, if any, critical attention has been given within this work to questions that would seem to be of fundamental importance to attempts at collaboration between disciplines: how is the disciplinary organization of knowledge to be understood, and more specifically, can HCI be adequately described in the simple pluralist terms in which it tends to characterise itself? The primary focus then is on the discipline of HCI. Utilising a theoretical model which considers disciplines as distinctive discourses which constitute their own domains of objects, I analyse the discourse and practice of HCI in order to explicate some of its underlying premises and assumptions, and to argue that it has, unavoidably, set many of the parameters within which contributing disciplines must operate. Texts, audio and video tapes, and ethnographic observation of instances of HCI practice form the empirical basis of the thesis. In addition, an analysis of some recorded human-computer interactions, which like the study as a whole, exemplifies an approach that differs from the prevailing sociological models within the field, is used to support the argument.

Acknowledgements

I am grateful to the following people who, in their different ways, helped me complete this research: to my partner Maggie for her support throughout, and to whom I dedicate the thesis; to my supervisors Hugh Robinson and Yvonne Rogers for their open-minded guidance; and to Steve Woolgar for his encouragement. Yvonne also helped considerably with the task of assembling empirical materials.

Contents

Chapter 1

Introduction

The discipline of Human Computer Interaction (HCI) aims, in the words of one of its practitioners, 'to develop or improve the safety, utility, effectiveness, efficiency, and usability of systems that include computers' (Diaper, 1989: 3); it speaks on behalf of computer users, advocating the importance of taking their needs, difficulties and viewpoints into account when designing computer software. It comprises a number of sub-disciplines, notably psychology, ergonomics and computer science. Sociology's role in this field has in the past been a comparatively marginal one, but the number of sociological contributions is increasing. This latter body of work claims to offer a distinctive view of the phenomenon which is HCI's central topic, computer use. However, for all that it asserts its difference from, and is critical of particular ideas within HCI, it has not subjected any of the discipline's fundamental assumptions to any critical scrutiny.

By contrast, although this thesis is also an explicitly sociological piece of work, it shifts the focus onto the discipline of HCI itself. Instead of simply asking what sociology has to offer *within* the given disciplinary framework of HCI, it suggests that there is much to be gained from a sociological analysis *of* HCI itself. To do this, it employs a mode of analysis which considers disciplines and their knowledge as distinctive discourses, each of which constitutes its own discrete domain of objects. It should be noted that this theoretical stance, stressing as it does the constructive character of discourse, also sets the thesis apart from other sociological work within

HCI. Moreover, it raises interesting questions about something which has been largely taken for granted within HCI as a whole: the nature and feasibility of interdisciplinary inquiry[1].

The purpose of focusing directly on HCI can be expressed in a number of ways. Primarily, the analysis of the way in which disciplines emerge and define their own domains is taken to be a topic of interest in its own right. HCI's comparatively recent emergence makes it particularly suitable for study since, it can be argued, the process of self definition is still discernible. To demonstrate that the discipline's domain of phenomena is, in important senses, its own creation is also to demonstrate the contingency of that domain. By this, I do not mean that there are no reasons for it taking the form that it does, but that these reasons are not provided by the brute reality or the logic of the phenomena themselves: the domain could be other than it is[2]. Secondly, viewing disciplinary formation in this way is instructive for developing an understanding of the prevalent form of sociology within the field: for it will be argued that sociological contributions can be characterised as sharing many of HCI's fundamental assumptions, and staying within its comparatively narrow terms of reference. Given that these terms of reference are contingent, then homologies between sociology and other work within HCI can be explained in terms of the interplay between different discourses.

The point of interest then is the disciplinary organization of knowledge in this area. Specifically, I am concerned with HCI and its discourse, and with sociology's role within it. In addition, the analysis should tell us something about the adequacy and plausibility of the framework that I have developed and used: for although the thesis has much in common with

---

[1] Whilst it is true that the coexistence of different disciplines (multidisciplinarity) can be distinguished from interdisciplinarity, I argue in chapter three that, in practice, claims within HCI for the desirability of the former tend to entail or imply claims for the desirability and feasibility of the latter.

[2] The contingency of these terms of reference can be illustrated by noting my own surprise on entering the field as a (naive) sociologist and realising that the discipline did not encompass, for instance, human-computer interaction as a cultural phenomenon ie. the increasing use of computers in all aspects of life considered as sociologically significant and interesting.

work in the sociology of scientific knowledge (SSK), central aspects of its theory and methodology, derived from the work of Foucault, are relatively untried in this area.

Although this approach represents a shift of focus, it does not imply that the thesis thereby turns its back on the possibility of contributing to HCI. The status of the thesis in this regard is largely dependent on its reception. It might be reasonably speculated that it would be read by some as too concerned with problems of theory and method, or more simply with sociology, to be considered a substantive piece of HCI work. The usual expectation in a multidisciplinary field seems to be that contributors should leave their intellectual baggage at the door and bring in a package of relevant findings; I would argue however that the challenge of delivering applicable knowledge necessitates an assessment of exactly what one has to offer in a new phenomenal domain, and more significantly, in a new discursive field. Indeed, this has been the case with the research reported here. Its original terms of reference were to make a sociological contribution to HCI. The shift of focus that I have described was partly a response to some of the issues and problems that arose in the course of the research: rather than treat these as obstacles, I have considered them as phenomena of intrinsic theoretic interest.

The other relevant issue here is how far a broadly 'meta-level' analysis would be seen to constitute a valid contribution. On this point, it should be noted that empirical materials suitable for analysis within the framework of HCI discourse have been collected and analysed. On the other hand, my analysis of them attempts to support my arguments about the discipline's contingent discursive structure and the extent to which sociology has conformed to it, by offering an account which exemplifies a different approach.

Therefore, whilst the thesis can be characterised by a double movement towards the analysis of, and the contribution to HCI, its putative status as contribution is subject to considerable interpretative flexibility and deliberately left as an open question[3].

---

[3] Interpretative flexibility of this kind being both characteristic of, and fundamental to, the theoretical framework I adopt.

1.1. Methods and materials

The theoretical framework that informs this research is discussed in detail in the following chapter, but it is important to briefly mention one of its central features at the outset, since it has an important methodological consequence. This is the view that language can be seen as prior to the individual who speaks or writes: utterances are to be understood, not as the expression of his or her thoughts, but as formed within available language games[4]. In particular, the mundane speech and practice of HCI personnel can be analysed in terms of the structure and assumptions of the wider discourse within which it is located, and which it helps to sustain.

The methodological implication of this is that a customary distinction, within sociological work, between texts and empirical materials breaks down. In analysing HCI, I relate mundane speech and practice to general features of its discourse, which are in turn postulated from the analysis of texts, and to a lesser degree, other instances of mundane practice; and I use textual materials to support assertions about the location of particular utterances within wider sets of assumptions. Text and talk are, in analytic terms, mutually supportive. The preliminary analysis of the discipline of HCI which precedes the presentation of 'field work' is therefore no less central to my argument, and no less 'empirical' than the later chapters: it should not be read as background to the main business of research. This again distinguishes my approach from much of the work in the sociology of scientific knowledge (SSK), where the impetus for its deconstructions frequently comes from an ironic contrast between what science says it does (in written texts) and what is seen to go on in practice.

---

[4] As implied here, one source for this apparently counter-intuitive approach can be found in Wittgenstein (1958: passim); more recently, work within continental theory has developed even more explicitly 'decentred' accounts of the human subject. This is discussed further in chapter two.

Apart from written texts, a variety of empirical materials are used. In the early stages of the research, the exclusive source of 'data' comprised video-tapes of human-computer interactions[5]. A number of have been transcribed and used in the thesis[6]. The interest of some of them is that they are readable as instances of HCI practice. As the focus of the research shifted in this direction, other types of materials which could be used to the same ends were assembled: notably, an audio-taped panel session from an HCI conference on possible criteria for valid contributions to HCI, ethnographic observations on an HCI project, and an audio-taped meeting of a committee which is attempting to define standards for icons and graphic symbols on computer interfaces, together with a number of supporting documents.

A word of qualification is needed on the scope of the thesis. One emerging area in which sociological work may be well placed to make a contribution is Computer Supported Cooperative Work (CSCW), where the emphasis is on the development of technology to support and facilitate collaboration in the work place; in general terms, the domain can be described as human interaction, mediated by computers. It has not been covered here: rather, I have concentrated on the 'mainstream' of HCI where attention is focused on the interaction of individual and computer, not only because I am interested in the predominant assumptions of the discipline as a whole[7], but also because the question of sociology's role within such a domain is both more contentious and more interesting.

---

[5] As with 'psychology', a single term is used to denote both the discipline and its object of study. I distinguish the two throughout by referring to the discipline as 'Human Computer Interaction' or more commonly 'HCI', and to the phenomenon as 'human-computer interaction'.

[6] The basis for selection is discussed in the following chapter.

[7] At the time of writing, this area remained a relatively small, though growing, area of interest within the discipline.

1.2. Structure

The thesis is organised as follows. In chapter two, the theoretical and methodological framework that underpins the thesis is set out. The discussion of relevant sources and materials makes use of a contrast between realism and scepticism as a way of signposting both the orientation of the argument, and its differences with other sociological work in HCI. It also discusses some of the problems and issues that arose in the course of the research which contributed to the development of the theoretical position outlined.

In chapter three, this framework is put to work in the description of the discipline of HCI. A broad outline of the field that it has defined and demarcated is given, and some central features and assumptions of its discourse are considered, paying particular attention to the way in which this avowedly multidisciplinary field is effectively dominated and shaped by the presuppositions of a core discourse - cognitive psychology. Chapter four then considers the sociological work in this field to date. It is not a conventional 'literature review' in the sense of simply listing and acknowledging prior research of relevance. For one thing, as I have argued, the relation of my research to this corpus is an ambivalent one, and more pertinent antecedents probably lie outside the field of HCI. More importantly, the chapter does not stand as a preliminary to my argument, but as a central part of it: I am interested in the form that this sociological work has taken, in *its* guiding assumptions, and in relating these to the discursive field into which it has moved. I suggest that dominant themes in HCI discourse are replicated in sociological discourse, the former creating the conditions of possibility for the latter.

Chapter five contains analyses of human-computer interactions. This is linked into the argument as a whole via its intention to demonstrate the possibility of a different form of sociological analysis from those that have predominated in HCI; by demonstrating that other forms of sociology are both possible and informative, I hope to underwrite my claim that existing work in the field has accepted by default the parameters of the core discourse of HCI[8]. The empirical

---

[8] Terminology is unavoidably misleading here: by 'accepted' I do not intend, in any sense, a conscious process involving individual sociologists, as will become apparent.

analysis of interactions is informed by the same approach to language that frames the description of disciplines, that is to say, its constructive character is emphasised and the interpretative flexibility that necessarily follows from such an approach is explored. It is suggested that this more sceptical or hermeneutic analysis still has much of value to say, and that certainty, realism, and an emphasis on the presentation of 'findings' does not represent the only path open to sociology.

Chapters six and seven then study some empirical instances of HCI practice. In chapter six, an HCI project on adaptive interfaces is the focus: looking at experimental design and interpretation, and at a consultation that took place to assess the adequacy and typicality of some prototype interfaces for the experiment, I draw out some further features of HCI discourse. In particular, the significance and peculiarities of its structural relation to design practice are discussed by relating some of the talk that goes on during the consultation to trends in HCI as a whole. Chapter seven focuses exclusively on a standards committee meeting in which HCI practitioners attempt to define standards for iconic interface design. It is suggested that the negotiation and management of manifold uncertainties that is apparent may be related to the emergent and still changing shape of the discipline; it is argued that a process of attempting to stabilise, define and regulate discourse is discernible.

Finally, chapter eight concludes the argument by assessing issues that have arisen in the course of the analysis, and considering some of the reflexive consequences of, on the one hand, stating a theoretical commitment to both the constructive character of language, and the specificity of different discourses, and, on the other hand, attempting to represent other discourses in the language of (a particular strand of) sociology: that is to say, I consider the implications of what I have been arguing for the adequacy and validity of my own account.

Chapter 2

Developing a theoretical framework

The primary purpose of this chapter is to set out and explain the theoretical approach, and the associated methods, that provide the basis for the arguments presented in the thesis. It is not a comprehensive discussion of a theoretical framework, since to attempt such a discussion would probably be of limited value in isolation from considerations of the way the framework is put to use with empirical materials. Rather, it attempts only to give sufficient initial guidance to the reader in order that he or she may begin to make sense of the logic of the arguments that follow: further theoretical issues will continue to be raised and discussed throughout the thesis.

Broadly stated, the approach taken can be characterized in terms of a general orientation towards discourse, which not only forms the chief analytic resource, but is conceptualized as an object of interest in its own right instead of as an essentially referential medium. This approach is drawn chiefly but not exclusively from work within ethnomethodology, post-structuralism (notably Foucault) and the sociology of scientific knowledge (SSK). However, the precise parentage of particular ideas is of much less significance than the central strand of argument which ties them together. This can be expressed in terms of a polarity between two epistemological positions, realism and scepticism. I subscribe to the latter in the sense that discourse is viewed as constitutive, not reflective, of the objects that it describes: consequently, limits are placed on the kinds of claims that can be made for the supposed superiority of accounts in terms of representational adequacy. By contrast, a realist epistemology strives for more

accurate representations of a presumed underlying reality[1]. One significant aspect of the sceptical approach to representation adopted here is that it distinguishes the arguments made not only from work in the mainstream of HCI, especially cognitive psychology and computer science, but also from other examples of ethnomethodology in the field: for example, as Woolgar (1987) has argued, claims for the superiority of ethnomethodological accounts of users' (practical) reasoning over the accounts given by cognitive science necessarily draw on a realist epistemology.

The issue of scepticism/realism is therefore the organizing theme for this chapter. Discussions of different theoretical traditions are focused primarily on those aspects which are relevant to this theme, and are necessarily and deliberately selective as a result. For example, ethnomethodology is discussed primarily in terms of the tension between realism and scepticism that, I argue, lies at its core, whilst discussion of Foucault's work underplays whole areas, such as the question of power, which are central to his concerns. I am neither attempting a theoretical synthesis, nor espousing theoretical pluralism for its own sake: the selection of materials is rather justified, I contend, by the need to build a coherent theoretical framework for the arguments that I wish to make.

Whilst I believe that some form of preliminary outline of the theoretical approach adopted is necessary, this form of textual organization does itself carry with it certain problematic theoretical implications, possibly commitments. It suggests that the theoretical orientation and the empirical material on which it is brought to bear are essentially separate, both temporally and conceptually: theory is in place prior to its application, and relatively unaffected by it. The inter-relation of theory and empirical research is far more complex than this. The theoretical position that I am outlining has been thought out in the course of doing empirical research, and

---

[1] Realism can also be paired with/opposed to relativism, or constructivism. These terms all overlap, though there are perhaps differences of emphasis, the former connoting issues of truth and the status of knowledge, and the latter the socially constructed character of phenomena: 'scepticism', as I have defined it, includes both of these senses.

partly in response to issues and difficulties raised whilst trying to assemble a body of materials: for instance, the orientation to using discourse as the fundamental empirical resource emerged in response to the difficulties of analysing silent interactions and to the comparative richness of an interaction in which the user talked freely. At the same time, decisions on what would constitute a suitable approach were made before some of the implications of epistemological scepticism had been thought through: for example, my rejection of conversation analysis was made partly on the basis that it would provide inadequate support for a critique of cognitivism. I have therefore included some discussion of the decisions, false starts and cul-de-sacs that have played an important part in the development of the framework that I am presenting[2].

2.1. Ethnomethodology

Ethnomethodology is a form of sociology which derives from the work of Garfinkel (1967). The term's intended referent is not a social science methodology but the methods and interpretative practices which people routinely employ in their everyday life in order to make sense of social actions, structures and interactions. There are however implications for the social study of these methods: for one thing, Garfinkel insists that these methods are a feature of all theorizing, lay or professional (Garfinkel, 1967: 75). The reflexive implications of this for the sociologist will be considered later in the chapter. Furthermore, a methodological injunction is implied via the assertion that any interpretation of social phenomena should take such practices as its primary focus and topic. The significance of this injunction for other forms of sociology is that the latter are charged with using these ethnomethods merely as unexplicated resources and failing to

---

[2] Silverman notes that the omission of such details is a prevalent feature of sociological research reports which 'read as if researching were just a matter of going from A to B, a direct path without diversions or doubling back from cul-de-sacs. As we all know, this is a gross misrepresentation of how most research is done' (Silverman, 1987: 1)

recognize them as the means by which social order, and the intelligibility , coherence and even visibility of social action are accomplished[3].

Garfinkel identifies a number of features and aspects of these ethnomethods which have been important for the subsequent development of the programme of ethnomethodology. I will not attempt any kind of comprehensive summary here but will briefly point to three key features of his analysis that have particular relevance for my arguments. All of these features, to differing degrees, can be subsumed under the general theme of practical reasoning: by practical reasoning is meant the sense in which presumed realities such as social order, intelligibility, similarity and distinctiveness, are the *practical* accomplishments of people in particular settings rather than *in principle* features of the world[4]. Such practical reasoning is frequently tacit.

Firstly, the documentary method of interpretation describes the pervasive practice of treating appearances, signs, documents as signifying a presumed underlying reality; further, that the presumed reality is used in order to interpret these appearances (Garfinkel, 1967: 78). For example, in one of Garfinkel's studies, subjects dealt with apparent contradictions and inconsistencies in what they took to be genuine counselling advice by constructing an underlying reality - what the counsellor really meant - which could resolve them (Garfinkel 1967: ch 3). Secondly, the indexical character of expressions is asserted as an essential feature of discourse, and therefore as an essential resource for its understanding: meaning has to be related to the settings in which expressions are used (Garfinkel and Sacks, 1970). This contrasts with the treatment of indexicality by logicians, and sociologists, as troublesome features of discourse which should be replaced with objective expressions (Sharrock and Anderson, 1986: 42-43). Thirdly, social members' accounts are 'constituent features of the settings they make observable' (Garfinkel, 1967: 8). Again this points to the need to relate discourse to the settings in which it

---

[3] To give an example, Silverman (1975) argues that much organizational sociology takes 'bureaucracy' as an objective organizational structure instead of examining the way in which the term derives its currency from its use in everyday life.

[4] This reading of ethnomethodology is derived from Ashmore (1989: ch 6).

takes place, and from which it cannot be analytically detached; it is also part of a wider argument that the orderliness of social life which is the sociologist's topic is the product of members' work. That is, members actively organize social action in order that it be intelligible, 'accountable' or 'observable-reportable' (Garfinkel, 1974: 17) to others. The accounts that people give of their actions will often be central to this process, but action, which may be silent, is also 'accountable' in the sense of displaying 'visible orderliness' (Sharrock and Anderson, 1986: 113).

Let us note two things that are suggested by this brief review of ethnomethodology. Firstly, the three features highlighted all have implications for the ways in which the analysis of discourse should be approached, although this is not their exclusive import. They all mitigate against the feasibility of analysing discourse as referential in function. Secondly, they all have implications not only for the object of study, social action, but for the process of study itself.

2.1.1. A theoretical tension

The widely differing approaches to sociological analysis that have developed under the broad umbrella of ethnomethodology, and especially the epistemological status claimed for their respective accounts, can be fruitfully related to a theoretical tension at the centre of Garfinkel's ethnomethodology[5]. This tension, which can be deduced even from the brief sketch presented above, is between scepticism and realism.

The realist impulse in Garfinkel's work derives from the insistence that the correct focus for analysis is the methods routinely employed by members in given settings, and from the related point that the sociologist should not attempt to remedy the indexicality of expressions within those settings by replacing them with objective expressions. This forms an important part of the basis for the oft cited distinction between ethnomethodology and other forms of sociology:

---

[5] I would stress that this assertion should not be construed as a criticism, since I believe that attempts to construct totalized and thoroughly consistent theoretical frameworks in sociology are inherently flawed, and that tensions within a theory can be productive. The aspiration to comprehensive and consistent frameworks is premised on a separation of description and described which is contested in this thesis.

analysis should begin with the examination of instances of practical reasoning in the settings in which they occur, and not rush to generalize these instances as examples of more universal phenomena conceptualized within *a priori* theoretical frameworks[6]. As a general statement of a research strategy and orientation, this would probably find a measure of agreement with most ethnomethodologists. However, in terms of epistemological guarantees it is given different credence. The realist strand goes on to infer from this orientation that members' methods can be, and are accurately represented by the ethnomethodologist. This is succinctly stated by Latour: 'Some of the followers of Garfinkel really believe that once all the methodological precautions have been carried out, the lived-in world of the competent members can be presented truer to life than in the gloss of a classical sociologist' (Latour, 1988b: 167). This realist aspiration is common to most ethnomethodological work, but it is particularly evident in conversation analysis, whose distinctiveness is said to lie, according to its practitioners, in its explication of the rules to which speakers themselves orient in the course of conversation.[7]

---

[6] A good example of the ethnomethodological critique of sociology's cavalier attitude towards the specifics of empirical materials is Sharrock (1989). The difference between ethnomethodology and other forms of sociology in terms of the latter's departure from the explication of the social members' own practical reasoning and action is spelt out clearly by Livingston (1987: ch 6), in which he argues that the analysis of pedestrian traffic flow with the aid of a camera positioned on the roof of a Manhattan building hides the 'order-productive character of pedestrians' practical actions and reasoning' (Livingston, 1987: 27). Livingston's confidence that a more grounded research strategy will bring out and more adequately represent the hidden character of these phenomena is however something I contest.

[7] A strong statement of this can be found in Wooffitt (1990), where it is emphasized that the goal of CA 'is not to furnish an academic or "outsider's" reading of some conversational sequence, but to describe the organized interpretations that *people themselves* employ' (Wooffitt, 1990: 15, original emphasis). Or again, Button's (1990b) argument for the primacy of turn-taking over rhythm as an analytic framework rests on the assertion that speakers manifestly orient towards turns (Button, 1990b). The use of conversation analysis within HCI is discussed in more detail in chapter 4.

Before moving on to look at the sceptical strand in Garfinkel's work, I wish to clarify the necessarily compressed remarks above by separating out some features of the realist strand. The aspiration to formulate representations that are more accurate and valid than those of other sociologies rests in general terms on a commitment to ground research and theorizing in empirical data. Beyond this however, claims for realism derive from two more specific aspects of Garfinkel's formulations. Firstly, the importance of settings for the understanding of social action, given for instance the indexicality of members' accounts, can be taken to suggest that a description which is mindful of contextual variation is more epistemologically grounded and certain[8]. Secondly, certainty is premised on the primacy that is given, implicitly or explicitly, to the standpoint of the member in any possible sociological analysis. This commitment to the subject's point of view is a pervasive although not essential feature of ethnomethodology which may be a residual feature of its beginnings in Schutz's phenomenology (Lemert, 1979). Whilst the critique of realism for which I will argue hinges on the uncertainty that attaches to any representation, it is also the case that this *a priori* commitment to the explication of the subject's point of view is a premise that I do not accept, and a key reason for making use of materials from outside ethnomethodology.

By contrast, Garfinkel's programme can also be read in a way that extends the analysis of practical reasoning as a fundamentally constructive activity in the face of irresolvable uncertainties to a full blown scepticism about the status of any representation, including ethnomethodological accounts. The most articulate advocate of this approach is Woolgar (1983; 1988a: ch 2). Garfinkel (1967) provides for this reading in at least two ways. Firstly, key aspects of the analysis of practical reasoning - the indexicality of accounts, the reflexive ties between accounts and the settings that they report, and especially the documentary method of interpretation - constitute an explicit critique of the 'realist mode of speaking' (Barnes, 1981: 484). Discourse is viewed as a constitutive part of the process whereby meaning is accomplished as a practical matter, and not as a signifying medium through which meaning is conveyed and

---

[8] This is the kind of claim that is made for ecologically valid, non laboratory methods in psychology (Forrester and Reason, 1990). I argue against some of the implications of this view in chapter 5.

objects represented; it is formative not reflective. Secondly, Garfinkel explicitly emphasizes that this analysis does not admit of a distinction between lay and professional methods of practical reasoning[9]. It is therefore necessary to treat ethnomethodological accounts as themselves subject to the same critique.

This sceptical strand is underplayed in most ethnomethodological work. Certainly ethnomethodology within HCI to date, notably Suchman (1987) and a number of conversation analysts (Luff et al., 1990), has taken a more realist approach to its subject matter. Again I wish to clarify that there are two aspects of what I am referring to as scepticism. In the first place, there is the view of discourse, or more generally representations, as constitutive rather than reflective which suggests that accounts cannot be treated as reflections of underlying realities. The acknowledgement of the significance of such a view for the theorist's own accounts arguably follows logically from this position, but is in practice a separate issue: this self-referential dimension is denoted by the term 'reflexivity'[10].

This way of interpreting Garfinkel makes many points of contact with contemporary work from other traditions. The critique of language as a mode of representation and the associated emphasis on its constitutive character is argued in different ways from within, to name but a few: philosophical pragmatism (Rorty, 1980), in the context of an argument against the validity and desirability of the central assumptions of post-Kantian epistemology; the intellectual movement

---

[9] Again however, there is some flexibility in the position he adopts. On the one hand, for example, Garfinkel's postscript to one of his analyses acknowledges that, in the light of new information that has come to light, his account should be read as an example of what it describes , that is 'a situated report' (Garfinkel, 1967: 288). On the other hand, McHoul notes that the suspicion of constructive theorizing and programmatic texts leads Garfinkel to neglect the significance of his analysis for written texts (McHoul, 1982). McHoul's work is a relatively rare example of an ethnomethodological study which places reflexive considerations of this sort at the centre of the analysis.

[10] The term has come to have a slightly different sense and usage from the way in which it was employed by Garfinkel to refer to the dialectical relation between account and setting.

of structuralism, which builds on de Saussure's (1978) insistence that meaning is derived from the relations between signifiers, and not from the arbitrary relation of signifier to referent; and following from this, the disparate group of theoretical approaches which are grouped together under the label of post-structuralism. For our purpose, the two key figures in this last group are Derrida whose 'deconstruction' of discourse is premised not on the indexicality of utterances but on the essentially unstable play of difference between signifiers (Derrida, 1978: 289), and Foucault who attempts to demonstrate the formative effects of the discourse of the human sciences (Foucault, 1970; 1972; 1977; 1979); Foucault's work is considered below. Derrida's work is particularly mindful of the reflexive implications of uncertainty for his own texts, hence the formal experimentation of much of his work. Reflexivity is also a concern of much work, not necessarily ethnomethodological, within the sociology of scientific knowledge, perhaps inevitably given its subject matter (see for example Ashmore, 1989, and Woolgar, 1988b).

If there is a unifying theme that justifies the inclusion of both Derrida and Foucault within the single category of post-structuralism, beyond the attention they give to the formative character of texts, broadly defined, it is a rejection of the analytical primacy of the human subject in explanations of discourse and its effects (Derrida, 1982: 14; Foucault, 1970: xiv). It is for this reason that I make use of this work, and Foucault in particular, in order to articulate the importance of discourse, considered as in some ways relatively autonomous of the subject who is located and formed within it. This provides a way of focusing language games in themselves (Wittgenstein, 1958: passim), a way which ethnomethodology does not provide, despite the frequent acknowledgement of Wittgenstein's importance by some of its practitioners (see for example Heritage, 1984). I argue that this shift of focus is productive for an understanding of the field of HCI.

However, before moving on to clarify the importance of discourse for my analysis and explicate the theoretical frame through which it is viewed, I want to briefly look at some of the tentative steps that were taken in a different direction in my research in order to illuminate some of the other reasons why a broad orientation to discourse has become of central importance to my thesis.

2.2. Why discourse?

I noted in my brief sketch of some central features of ethnomethodology that the accountable character of social action should not be taken to imply that only those settings involving talk or texts were amenable to analysis. Indeed Sharrock and Anderson's review of the field emphasizes the recognition of the display of '*visible* orderliness' as one of its fundamental insights (Sharrock and Anderson, 1986: 113, my emphasis). It may therefore be legitimately asked why my research into human-computer interaction, initially conceived as an ethnomethodological study, has taken discourse as its primary material and resource. I will outline here two approaches to the study of such interactions which were considered, and in one case attempted, and then rejected. The difficulties involved in the analysis of silent action were a influential factor in the formulation of the approach described in this chapter. It is worth noting that some of the grounds for rejection were based upon a prior commitment to countering cognitivist theories of human reasoning with a more 'situated' account (Suchman, 1987); to this extent, they were framed within a set of realist assumptions to which I no longer subscribe.

The first approach that was considered was to use conversation analysis (CA) to study human-computer interactions: that is, to examine such interactions as forms of dialogue and identify their dynamic structure. The question of CA's possible relevance in this area is considered in more detail in the review of some sociological work in the field in chapter 4, but I will briefly note here that this approach was rejected for two reasons. The first was that the application of CA in a domain which is not characterized by normative reciprocal commitments would tend towards an account which stressed the determinate character of rules[11]: this would not only be counter to the spirit of conversation analysis, but would fail to realise the radical potential of ethnomethodology to provide an account of situated practical reasoning, which I took to be the chief purpose of the research at the time[12]. The second reason was the more general objection

---

11 Button (1990a) argues a similar point.

EX12

HIGHER DEGREES OFFICE

LIBRARY AUTHORISATION FORM

STUDENT: GEOFF COOPER SERIAL NO: M7039366

DEGREE: PhD.

TITLE OF THESIS: REPRESENTING THE USER: A SOCIOLOGICAL STUDY OF THE DISCOURSE OF HUMAN COMPUTER INTERACTION

I confirm that I am willing that my thesis be made available to readers and maybe photocopied, subject to the discretion of the Librarian.

SIGNED: G. Cooper DATE: 8/5/92

that in imposing an *a priori* framework on the data, it would necessarily prescribe a characterization of it which would be premature[13].

Given this commitment to a method of analysis which was appropriate to the domain of its application, work going under the broad rubric of 'studies of work' seemed to offer a more useful avenue to follow (see Heritage, 1984: ch 9). There is no room here to discuss to what extent these studies represent a new development within ethnomethodology or merely a continuation of earlier work. Its distinctive emphasis however is on the desirability of avoiding the problem of representation whereby 'the lived realities of occupational life are transmuted into objects suitable for treatment within the accounting practices of professional social science' (Heritage, 1984: 301). In this respect it is squarely within the realist approach to representation which is contested here. The significant aspect of these studies for me was however different: some of them comprised analyses of silent embodied actions. For example, Sudnow (1978) studies the acquisition of embodied competences in learning to play jazz piano; however, the introspection that forms a key element of the study made its wider applicability doubtful. The more promising example was Lynch et al's (1983) discussion, particularly the account of Schrecker's study, in which the latter took a job as a laboratory assistant and videotaped himself at work: his arranging and organizing of materials on the laboratory bench was then analyzed as a visible instance of practical reasoning. Beyond the aptness of the method of assembling empirical materials - using video - this study suggested that a critique of cognitivist assumptions could be based on an emphasis on the embodied nature of practical reasoning.

---

[12] Of course, it is possible to construe Suchman's work, which exemplifies the kind of critical account that I had in mind, as an example of conversation analysis. I would argue that while it draws on many of CA's insights, it is distinct in important respects. This is discussed further in chapter 4.

[13] The specificity of CA for the analysis of conversation is forcefully argued by Sharrock and Anderson (1986: ch 6). Bilmes (1988) argues against this position and asserts the general relevance of the CA approach for the study of action and interaction.

Accordingly I began, like Schrecker, by videotaping myself installing and attempting to use an unfamiliar piece of software: Hypercard on the Macintosh. Subsequent inspection of the tapes proved promising: it was possible to make observations about the spatial arrangement and organization of manuals and other relevant documents on the surface of the desk[14], the deployment of the focus of attention between different materials (on-screen and off), the role of auditory phenomena in orchestrating shifts of attention, and embodied competences that were visible in my attempts to get the software running. On the basis of this initial attempt, a number of silent and solitary interactions involving other people were videotaped. After studying these, I abandoned this approach.

The main problem with these empirical materials was that they appeared to offer insufficient analytical leverage for a distinctive, and interesting, sociological account. For one thing, very little could be observed in the majority of them that could be construed as supporting the critique of cognitivism which was my chief concern at the time[15]. More fundamentally, significant doubts were raised about the possibility of making a distinctive sociological contribution to the field of HCI with such materials: in the absence of talk, texts, or any kind of interaction between people, the only possibility seemed to be some form of purportedly naturalistic description[16], and moreover a description which would be hard pressed to explicate the social character, however broadly conceived, of what was taking place.

---

[14] There are some similarities here with the work of Malone (1983), whose study of desktop organization is said by some to have played a pivotal role in the adoption of a desktop metaphor in computer system design (Norman, 1988: 221).

[15] This is arguably an instance of the cavalier approach to empirical materials denounced by Sharrock (1989), in that prior concerns guided their selection or rejection. Practically however, such selection is an inevitable and necessary part of the research process.

[16] Indeed Heritage goes even further than this in characterizing the studies of work programme as an 'observational science' (Heritage, 1984: 298), a description which Lynch (1986) contests.

These initial attempts therefore helped to direct the research towards an interest in discourse in two ways. Firstly, the occurrence of one interaction in which talk arose spontaneously between researcher and user provided a much richer source of empirical materials for sociological analysis. Secondly, my dissatisfaction with naturalistic description of bodily actions in terms of its possibilities for making a distinctive sociological contribution to the field of HCI focused attention onto the distinctive character of different academic discourses. The fact that discourse can be seen as distinctive, and as constituting distinctive objects, suggests that there is no simple atheoretical observation prior to the construction of an account: the gaze of the researcher is framed within the discourse of his or her discipline. It also suggests that this formative character of disciplinary discourses is a significant topic in itself, and one with important implications for a multi-disciplinary field such as HCI.

2.3. The analysis of discourse

As has already been noted, ethnomethodology provides a number of theoretical tools for the analysis of discourse. In this section, I outline some other theoretical materials that can supplement ethnomethodology with respect to this topic, and which inform this thesis. Before doing this, I will briefly summarize the significance of those features of ethnomethodology that have been mentioned for the analysis of discourse in terms of two themes. Firstly, language is constructive and non-referential. Secondly, when studied in relation to the settings in which it occurs, language can be seen to *do* things; it is a functional part of those settings. As Potter and Wetherell point out, ethnomethodology shares this emphasis with speech act theory (Potter and Wetherell, 1987: 32).

It should be stressed that there are a bewildering number of quite theoretically distinct approaches to the study of language that all go under the label of discourse analysis, and the approach adopted here, although in some respects a relatively eclectic one, has very little in common with the majority of these[17].

---

[17] For a brief summary of some of the key variants see Potter and Wetherell (1987: 6-7).

2.3.1. Discourse analysis in the sociology of scientific knowledge

Discourse analysis has played a significant part in the SSK project of deconstructing the claims that are traditionally made for the objective and privileged status of scientific knowledge. To the extent that such claims tend to be realist in character, discourse analysis in this field constitutes a critique of realism; however, there are considerable differences of emphasis in this respect, and it is no more a homogeneous body of work than is discourse analysis in general[18]. The common concern is the treatment of scientific discourse as a topic in its own right and not as an index to other phenomena such as actions or beliefs (Mulkay et al, 1983: 196). I shall identify three key themes before considering some of the limitations of this body of work.

Scientific discourse can be analysed to bring into focus the textual and rhetorical devices that are used in it. Such devices are seen as crucial to the achieved authority of the scientific text, and to the construction of a referent; in Woolgar's words, science is 'construed as a discourse in and through which the prior existence of objects (things) is accomplished' (Woolgar, 1988a: 67-68)[19]. For example, Latour (1987) analyzes the effects of modalizers in subverting or reinforcing the claims made within scientific literature, whilst both Woolgar (1980) and Gilbert and Mulkay (1984) point to the importance of the passive tense for sustaining a sense of the scientist as responding to and apprehending the pre-existent empirical world: in Gilbert and Mulkay's terms, the 'empiricist repertoire'.

Another focus for analysis, which is in many ways unique to this field, is the material significance of texts. This is largely due to Latour (1986; 1987), who characterizes scientific work as a process of creating inscriptions. Latour points to processes whereby initially provisional

---

[18] By the same token, my treatment of 'realism' as a unitary epistemological position is a considerable over-simplification: for an idea of the potential complexities of the term see Fuller (1988: ch 3).

[19] The study of rhetorical devices in scientific discourse does not always take such an anti-realist position. For example Keith is careful to distance his analysis of scientific rhetoric, that is the persuasive properties of scientific texts, from any epistemic claims (Keith, 1990: 365-366).

knowledge becomes solidified and authoritative through continual inscription and re-inscription, identifies the importance of establishing associations and connections between texts, and, in line with his view of explanatory power as the capacity to minimally and efficiently represent as many things as possible in order to act on them at a distance, argues that scientific progress is largely a question of developing more refined inscription technologies[20].

Finally, there is a concern with variation and regularities in scientific discourse, as measured in relation to different contexts of use (Gilbert and Mulkay, 1984). For example, whereas the empiricist repertoire, noted above, may predominate in written texts and in formal settings in which scientists seek to underwrite the validity of their work, on less formal occasions this is supplemented by a contingent repertoire which emphasizes the role of human agency and contingency in scientific work (Mulkay et al, 1983: 197).

Given my interest in the significance of the formative properties of discourse, and in particular the discourses of HCI and its contributory disciplines, these elements are suggestive and useful. However, there are certain shortcomings associated with this work for the arguments I wish to make. Firstly, the distinction between formal, official texts and what 'really' goes on in the informal day-to-day business of scientific work that is implicit in much of SSK is an unsatisfactory one in my terms: not simply because it partially trades on the realism that is the subject of its critique (see Woolgar, 1988a: 10), but more fundamentally because it leaves out of focus the relative autonomy and formative qualities of the scientific discourse which frames practitioners' speech and writing, in formal and informal settings[21]. Secondly, there is

---

[20] It should be noted that in Latour's work, 'inscriptions' form a wider class of objects than simply written texts: for example, photographs, diagrams, graphs and the output of technologies developed and used in science for rendering visible and measuring phenomena.

[21] To clarify my position here, the distinction between formal and informal discourse can be of analytic value as Gilbert and Mulkay have shown (1984), though, even here, the epistemological prioritising of informal discourse seems to be difficult to avoid. My stress on continuities rather than differences between the two settings is intended as a strategic analytic move, not a denial of discernible variation.

comparatively little attention given to the differences between sciences; the emphasis is on science in general, and there is no sense of the role of discourse in defining a distinctive field of study[22]. Thirdly, even though the idea that language is the expression of the individual's prior thoughts is undermined, for example through the stress on variations and regularities in relation to social settings, the implicit model is still one in which the individual is the instigator of language use, albeit in response to the parameters of social setting or in order to achieve rhetorical effects. It is in relation to these shortcomings that Foucault's work is of value.

2.3.2. Foucault

There are considerable difficulties in summarizing Foucault's work because, as with many other modern thinkers such as Marx, Wittgenstein, and Heidegger, most interpreters insist that there are fundamental differences of emphasis if not discontinuities between his early and late work. His only explicit attempt to define discourse and its analysis (Foucault, 1972) was immediately followed by case studies which departed from this formulation in fundamental ways (Foucault, 1977; 1979). In the former, he analyses the rules that govern the formation and structure of discourses; in the latter, the terms of analysis are significantly broadened to encompass the social effects of discourses, whilst the importance of rules recedes. I shall ignore these differences because most of the points that I want to bring out are central to both periods: chiefly the insistence that discourses be treated not 'as groups of signs (signifying elements referring to contents or representations) but as practices that systematically form the objects of which they speak' (Foucault, 1972: 49)[23].

A crucial distinction between Foucault's conception of discourse and the approaches reviewed above is that he is concerned exclusively with what Dreyfus and Rabinow call 'serious speech acts' and the wider formations within which they occur, and not with mundane speech (Dreyfus

---

[22] There are exceptions here, one example being Bazerman's study of the stylistic variations between three examples of academic discourse (Bazerman, 1981).

[23] The plural formulation 'discourses' used in this paragraph itself suggests an important distinction between Foucault's approach and some of the work that has already been discussed in this chapter.

and Rabinow, 1982: 48): that is, with the institutionalized forms of discourse which he characterizes as the human sciences, whose legitimacy rests on their claim to speak some form of truth about, and on behalf of, the human subject. The proliferation of such discourses, he suggests, are a characteristic feature of modern western societies. His analyses explicate the distinctive character of some of these discourses at particular historical conjunctures: for example philology, biology and political economy (1970), criminology (1977). Of fundamental importance is his assertion that the distinctive character of a given discourse, that is the relative unity of the statements that it comprises, does not derive from the objects of study: such objects are the product of discourse not its foundation[24]. Rather, a discourse actively maps out a terrain of possible and valid statements, sets the boundaries of that terrain and constitutes the legitimate objects of study within it. This is in large measure a process involving the assertion of professional self-interest by means of the definition of an exclusive area of expertise, although Foucault is emphatic that this is not a conscious or conspiratorial process, but a general and relatively autonomous feature of the production of knowledge *per se*[25]; discourse precedes the conscious intentions of those who make statements.

It should be noted here that despite the considerable influence that Foucault's work has had within the social sciences in recent years, his work has not been taken up in any significant way as a viable approach towards the analysis of scientific disciplines and their development, but

---

[24] Foucault's methodological treatise (1972) also dismisses the significance of conceptual coherence, stylistic conventions, and reference to a common theme. My use of his ideas is loose in that I give some significance to the last two of these; but arguably, his later work also takes a more relaxed position on this typology.

[25] This begs the question of how far Foucault's method could be generalized to the natural sciences, a matter on which he is equivocal: it is an empirical question (Foucault: 1988: 106-7). Since I see HCI as a human science in important respects, this question is not germane to the use of these ideas in this thesis. Furthermore, the debate within SSK about the limits of the social throws the distinction between human and non-human sciences into question (see chapter 5).

has rather been seen as a theory of power and social regulation: in this respect, my use of these ideas is novel[26].

Further aspects of Foucault's analysis can be illustrated with reference to 'Discipline and Punish' (Foucault, 1977). Here the shift from an apparently more primitive and brutal form of punishment to the supposedly more humane modern penal system of the present day is studied; and this is correlated with the emergence and growth of the human sciences over the same period (19th and 20th centuries). Not only are these discourses, as I have noted, seen as prior to the speaking subject and consciousness: since the objects of study in the human sciences are people, these discourses actively constitute people in particular ways. Thus, criminology and other discourses constitute the law-breaker as a criminal, formulate the criminal's 'essential' nature in terms that imply their own unique capacity and expertise to understand him or her, and in so doing assert the legitimacy and boundaries of their own field of knowledge. The subject is an effect of discourse, and placed in certain subject positions - in general one of deference to expertise - by it[27].

Two further points about this study are of relevance to my position. Firstly, as has already been implied, Foucault challenges the humane credentials of this modern form of punishment. It is more controlled and more regulative than older forms, and via the claims made by the human sciences, it makes every aspect of the subject's life available for study, examination, regulation and judgement; and it should be noted that Foucault sees this rationalization process moving beyond the domain of penal correction to all aspects of the wider society. At the same time, and as an integral part of this process, the human sciences see themselves in a humanitarian light: their aims are to understand the criminal, and to reform his or her conditions. Their liberal and well intentioned rhetoric is part of the process of whereby disciplines - and Foucault sees both

---

[26] Although Foucault's work is mentioned within some SSK texts, the only paper for which it forms the explicit basis is Hicks and Potter's (1990) discussion of citation analysis.

[27] A clear example is the constitution of the subject position of patient within medical discourse, and some of the limitations implied in terms of interactional and other rights (Silverman, 1987).

senses of the term as inextricable - are established. Secondly, it should be stressed that Foucault's sense of discourse in this later work encompasses more than simply language: the architecture of prisons, and the development of forms of corrective technologies are seen as part of discourse just as surely as are theories of correction.

Let me briefly sketch out how such an approach might be used to analyse HCI as a discourse, at this point in time: it will be discussed in more detail in the next chapter. Objects defined by this discourse, as a way of asserting its own unique sphere of expertise in opposition to, say, software engineering, would include 'the user-interface' and 'the user'. The criteria of validity for statements within the field might include some concept of usefulness for enhancing 'usability', and probably exclude, for example, purely aesthetic considerations. The liberal rhetoric that characterizes much of the field, for instance commitment to the project of 'user-centred design' could be correlated with the improvement of performance and efficiency in the work place. Finally, material aspects of and changes to the design of computer systems that follow from HCI recommendations would themselves be conceivable as part of HCI discourse.

2.4. Discussion

2.4.1. Theoretical tensions

I have presented a number of ideas from ethnnomethodology, SSK and the work of Foucault. These ideas cohere around certain broad themes, of which the rejection of a realist epistemology is the most important: discourse is analyzed as a topic of interest in its own right, as constitutive rather than reflective. It is neither reflective of the objects it describes, nor readable as the expression of the prior (pre-linguistic) thoughts of the speaking subject; indeed, the subject is in important respects a function of discourse. A further related theme is the emphasis on discourse as a material reality (Silverman and Torode, 1980), as present in ethnomethodology's stress on what talk does, Latour's concept of inscriptions, and Foucault's inclusion of both language and artefacts within a concept of discourse and discursive practice. Having said this, it will be apparent that there is no possibility of a seamless theoretical synthesis of some of these ideas,

and I have no wish to attempt one. There are on the contrary, a number of tensions between some of these ideas, especially between Foucault and ethnomethodology, which I believe to be potentially productive.

One set of tensions derive from Foucault's historical mode of analysis. This might appear to offer limited purchase on the analysis of speech, given his exclusive concentration on the written texts of the human sciences, and thus fundamentally at odds with ethnomethodology's analysis of mundane speech. However, both Silverman (1987) and McHoul (1986) have attempted with some success to extend Foucault's ideas to inform an empirical analysis of mundane speech: theoretical fidelity being, as here, less important to both than analytic utility. Moreover, the historical dimension in Foucault's argument can be used to justify such an extension: since he suggests that 'serious speech acts' - institutionalized forms of discourse which make implicit claims for their own expertise - are proliferating in modern western societies, it can be argued that mundane speech is increasingly permeated by such discourse[28]. Consequently, there are many situations, institutional and relatively informal, where such an analysis can be useful. In my analysis of HCI discourse, I therefore treat texts and spoken interactions between HCI practitioners within the same analytic framework; whilst there is also some use of Foucault's concept of subject positions in an analysis of mundane interactions with computers.

The contrast between Foucault's historical analysis of the constitutive character of specific discourses and Woolgar's (1983; 1988a) critique of representation is an instructive one. Viewed from Foucault's perspective, Woolgar's is a universalistic and idealistic formulation of the essential character of any form of representational practice which is unable to do justice to the specific and variable character of different discourses. For example, I suggest in chapter 7 that it may be the fact that HCI is a relatively new discipline, an emergent discourse, that makes the application of Woolgar's approach particularly apt. Conversely, a consequence of Woolgar's

---

[28] The precise relationship between expert knowledge and everyday lay discourse has been largely ignored, as here, by those sociologists who have attempted to use Foucault's ideas. A notable exception is Walker (1988).

universalistic position is a close attention to the significance of scepticism for his own representational practice, a dimension which is lacking in Foucault's work[29]. Each can be used to play off the other.

Finally, there is the tension between what in Foucault looks from my brief description like discourse as a determinate system, and ethnomethodological opposition to such formulations which cast the social member as a 'cultural dope' (Garfinkel, 1967). This is a more complex difficulty, but the differences are not as great as might appear. As McHoul argues, both writers oppose mechanistic structuralism with its connotations of rigid determination, and furthermore share an emphasis on the details of discursive practice over and above consciousness (McHoul, 1986). To say that the speaking subject is always within discourse may deny the validity of the phenomenological conception of the speaking subject, but it does not imply that the subject is not able to move between discourses, to use one to challenge another, to make use of the limits and constraints of discourses, and to take up different subject positions[30].

2.4.2. Some implications

Having sketched the outlines of a theoretical approach, I will conclude by indicating some its implications both for the study of human computer interaction and the study of the discourse of HCI.

Inasmuch as this thesis espouses a version of ethnomethodology, it is a radically non-cognitive form of ethnomethodology. That is, it does not propose an account of the way in which people reason in the course of using computers, or infer from their discourse the ways in which they interpret a computer system. This sets it apart from cognitive psychology, but also from

---

[29] Dreyfus and Rabinow (1982) contest this, but chiefly in terms of the choice of methodology: there is however no acknowledgement of reflexive concerns within Foucault's texts. Baudrillard's critique of Foucault can be read as centring on precisely this issue (Baudrillard, 1980).

[30] As Silverman notes, the speaking subject is not coterminous with the individual: two speakers may exchange subject positions (Silverman, 1987: 196).

Suchman's (1987) account of situated action. This is the consequence of the epistemologically sceptical position adopted with respect to the interpretation of discourse, and in particular the concept of decentred subject who is formed by discourse. Lemert (1979) suggests that without this view of the subject, ethnomethodology remains 'a sort of social psychology' (Lemert, 1979: 303)[31]. When we look at discourse around the computer, discourse itself will be the topic; there will be no attempt to go beyond it or use it to infer processes of reasoning, cognitive or situated.

The concept of discourse as bodies of statements that each constitute their own objects has implications for a multi-disciplinary field such as HCI. In the first place, it suggests that attempting to specify a relationship between the objects within different contributing discourses may be misconceived. For example, it is impossible to say what is the relationship between cognitive psychology's concept of a 'mental model (Gentner and Stevens, 1983), and ethnomethodology's 'practical reasoning' as they have been formulated within different discursive fields; it is therefore not feasible to attempt to challenge one with another. I will argue in the next chapter that there is a tendency in multi-disciplinary fields for a core discourse, or discourses, to predominate and for others to be drawn within their terms of reference. For example, the question of whether the documentary method of interpretation refers to the ways in which people reason about the relation between appearance and reality, or whether it refers to the ways in which they talk about it does not arise in the same way within sociology but is a point of some importance when sociology is looked to to provide correctives for decontextualized accounts of cognition. Similarly, sociological discourse can become conflated

---

[31] However, it must be said that there are key ethnomethodological concepts which, for all Garfinkel's assertions that 'nothing of interest is to be found under the skull' (quoted in McHoul, 1986: 68), look like proposals for the way in which people reason; a prime example being the documentary method of interpretation, even if it also carries within itself the means of its own deconstruction. It is notable however that Garfinkel's empirical exemplification of this phenomenon can be read in terms of the resolution of inconsistency via the presumption of underlying realities *within accounts* (Garfinkel, 1967: ch 3): on such a reading, interpretation is a purely discursive practice.

with other approaches which it may resemble in some respects but be fundamentally counter to in others; or more accurately, where there is interpretative flexibility (most places), interpretation will tend towards assimilation. This is one of the primary motivations for my shifting the focus onto the wider discourse of HCI itself. To do sociology within this area is to operate within a discursive formation whose parameters, whilst not fixed, are being established, and which place constraints on what kind of contribution may be made, calls for disciplinary pluralism notwithstanding. The analysis of discourse presented here therefore serves to both provide an alternative approach towards the study of interactions to those which prevail in the field, and, more significantly, to focus attention on the structure of the field and its significance for the character of the analyses to which it is an alternative.

However, the reflexive aspects of this caution against the view that the analysis of discourse can claim the kind of authoritative meta-level overview that I appear to be suggesting. It is a rather simply a view of HCI from within the terms of one discipline, sociology, although it draws on a current of thought that flows through many disciplines within the humanities[32]. A further consequence of the reflexive implications of the sceptical position taken towards representation concerns the status of the kinds of claims that may be made. It is not tenable, given the necessity of interpretative flexibility, to present 'findings' of the sort expected by a discourse with aspirations towards the assembly of a cumulative body of knowledge such as HCI, or for that matter conversation analysis. However, this does not imply that such an approach cannot make a contribution that is of value to HCI in other respects.

---

32 Klein points out that interest in 'the common problem of how language constitutes reality' has lead to the wider application of such techniques as deconstruction in the fields of history, philosophy, anthropology, literary studies and sociology (Klein, 1990: 268). But not cognitive psychology!

Chapter 3

The discipline(s) of HCI

The last chapter began to outline a conception of a discipline, largely derived from Foucault, which suggests that it can be characterized as a discourse which constitutes its own objects of study, thereby demarcating its work from other discourses and establishing its legitimacy and its own distinctive character. The central aim of this chapter is to look at HCI from this perspective: that is, to give a broad outline of the field that it has defined, and is still in the process of defining for itself, and to highlight some central features of its discourse. The purpose of this is, in general terms, to give a sense of the current shape and character of the field; more specifically, the intention is to consider some of the implications of viewing HCI discourse as constitutive and to illuminate some of the issues at stake for aspirant contributory disciplines, notably sociology. These issues will be explored further in subsequent chapters.

The materials which form the empirical basis for this discussion are a number of key texts, and transcribed extracts from a tape-recorded panel session at the Interact '90 conference entitled 'New approaches to theory in HCI: how should we judge their acceptability?', hereafter referred to as Data 2.4[1]. These materials are not presented as a comprehensive review of the entire field's literature, but as a selection which highlights fundamental themes and assumptions. In order that the process of selection should not be completely self-confirming, I

---

[1] See the appendix for full details of the data used and referenced in this thesis.

have made a point of giving some prominence to papers which attempt to give some kind of overview of HCI, that is, to examples of self-representation within HCI[2]. Beyond this, selection has been guided by a wish to capture the current state of an evolving discipline, and by an interest in those areas of HCI to which sociology has made, or might wish to make a contribution[3].

To discuss HCI as a discipline presents certain conceptual and terminological problems. On the one hand, it is central to my argument that HCI can be fruitfully viewed as a discipline in itself. On the other, it comprises a number of contributing disciplines such as computer science and cognitive psychology, or even cognitive science which can in turn be further decomposed. Rather than try to construct what would amount to a complex model and associated terminology such as 'discipline', 'subdiscipline' and 'supradiscipline' (Klein, 1990: 268), I use the single term 'discipline' throughout. In this, I follow Long (1989: 6) who refers to any and all of the following as disciplines: HCI, cognitive ergonomics, and the latter's contributing bodies of knowledge from within cognitive science. Thus, the assertion that HCI is multi-disciplinary is, in my terms, perfectly compatible with the assertion that it is also a single discipline[4]. However, the conception of a discipline used here differs from Long's in important respects. The chief difference is that he takes a discipline's formation and existence to be a response to the prior existence of the phenomena which fall within its scope (Long, 1989: 5), and more specifically as

---

[2] It may be objected that such overviews are not necessarily representative of the assumptions that inform HCI practice, and that might be found in other more specifically problem-oriented papers. Whilst I have attempted to focus on recurrent features of HCI discourse, the validity of my selection and the plausibility of my arguments must remain a matter for the reader.

[3] However, demarcating areas of sociological interest is not simply a question of distinguishing the 'social' from the 'technical'. Much of the work in SSK, and in particular Lynch (1985), has sought to undermine this distinction, whilst the discussion that follows in chapter five also implies that the domain of the social should not be restricted to interpersonal relations.

[4] This is not to say that I regard the collaboration of different disciplines within a single field as unproblematic, as will be apparent below.

an attempt to solve the general problem which defines the scope of such phenomena (Long and Dowell, 1989: 12); by contrast, I consider disciplinary formation to be an active and discursive process in which phenomena and problem are created, so establishing the distinctiveness and therefore legitimacy of the discipline. It should however be remembered that the conception of discourse that I am using is not restricted purely to what is written and said: in these terms disciplinary formation, considered as a discursive process, therefore encompasses practices.

I begin by giving a very brief overview of the discipline of HCI, and consider some problems associated with multi-disciplinary fields in general, before moving on to examine the discourse of HCI. I then briefly discuss possible roles for sociology within HCI, a theme taken up in more detail in the following chapter.

3.1. The discipline of HCI

A leading practitioner provides the following description of the goals of HCI: 'to develop or improve the safety, utility, effectiveness, efficiency, and usability of systems that include computers' (Diaper, 1989: 3). Long and Dowell describe its 'general problem' as that of 'designing humans and computers interacting to perform work effectively' (Long and Dowell, 1989: 14). Its primary practical focus is the user interface, a term which, like many others within HCI, resists easy definition but for the moment can be roughly conceptualized as 'the medium through which a user communicates with a computer' (Benyon, Preece and Rogers, 1990: 13). Whilst, as we shall see, many HCI practitioners resist the restriction of their work to interface design issues, it remains the defining centre of HCI practice at present.

HCI is multidisciplinary, and this is commonly perceived as a necessity by its practitioners, given that its objects of study include both human behaviour and the engineering of artifacts[5]. A

[5] To give a few examples of the importance that HCI places on its multidisciplinarity: 'a multi-disciplinary approach is essential (Winder, 1991: 28); 'HCI needs to be a church of broad foundations if it is to achieve

precise specification of its core disciplines is difficult given that there is a degree of variation within its own accounts. Diaper (1989) names psychology and computer science, with ergonomics (or human factors, its American equivalent) as one of a number of subsidiaries; Long identifies 'software engineering (and computer science)' as one, and ergonomics as the other, with cognitive psychology named as one of the cognitive science disciplines that contribute to a subset of ergonomics, cognitive ergonomics (Long, 1989: 5); another summary suggests that cognitive psychology, computer science and ergonomics constitute three core disciplines (Benyon, Preece and Rogers, 1990: 20). Instead of trying to adjudicate between these different formulations, one way of approaching the definition of the discipline's structure is by briefly sketching some aspects of its historical development and emergence: this approach does not resolve variation, but it does give some insight into how such variation is possible. However, caution is urged in the interpretation of this outline, which is drawn from accounts of its own history by a number by HCI practitioners (Baecker and Buxton, 1987; Shackel, 1990; Grudin, 1990). They all, to different degrees, see continuities between early work in ergonomics and computer science which predates the establishment of a separate discipline, and later work within HCI itself. On this account, the problems of usability have always been there in obscure and barely recognized forms, and the establishment of HCI is seen, in almost Hegelian terms, as a dawning of the awareness and coherent formulation of these problems[6]. In other words, it represents an account of disciplinary formation which is substantially at odds with the one for which I am arguing. However, it both serves the purpose of providing a context for different representations of the structure of HCI, and can be viewed, in itself, as part of the discursive construction of a disciplinary field.

Proto-HCI work begins, we are told, in the late 1950's. According to Shackel (1990), and Gaines (1984), Shackel's paper 'Ergonomics for a computer' (Shackel, 1959) is the first paper in the

---

real success (Diaper, 1989: 5); 'We are prepared to take on board any discipline, any approach that helps' (Draper and Norman, 1986: 2).

[6] This is an oversimplification in the sense that these accounts relate the emergence of HCI to the historical development of computers themselves, and in particular the increasing numbers of non-specialists using them.

literature. Subsequent work in ergonomics was sporadic throughout the sixties, but in 1969 the first major conference was held ('International Symposium on Man-Machine Systems') and the first relevant journal founded ('International Journal of Man-Machine Studies'). Within computer science, Martin (1967) devoted a chapter of his book on data processing to the question of different styles of 'conversation' with computers and subsequently wrote a book on the subject (Martin, 1973). Work continued to develop within both fields, but chiefly ergonomics, throughout the seventies, during which period psychology began to make a contribution. Weinberg's 'The Psychology of Computer Programming' (1971) is probably the first key text, and its subject matter reflects the fact that a relatively high proportion of users at this time were programmers; subsequent work in psychology was largely concerned with this topic until the early eighties[7].

The emergence of HCI as a self-consciously distinct discipline is said by Grudin (1990) to occur in 1982; and this appears to be a crucial date in many accounts. In the USA, the Gaithersburg conference was held, and lead to the establishment of SIGCHI (Special Interest Group on Computer Human Interaction). In the UK, the Alvey programme was launched. In both cases, the definition of a distinct field of interest is largely the work of cognitive scientists, and in particular cognitive psychologists[8]. For example, the Alvey Report, in large measure a response

---

[7] The frequent assertion, within HCI and other areas of computer science, that most users were programmers prior to the advent of the micro computer in the eighties should be treated with a degree of scepticism. Although it has some validity, it reflects the relative neglect of commercial data processing practices within parts of the academic computer science community: for example, the most widely used programming language, COBOL, is rarely taught in universities (although it is in polytechnics). Consequently, the large numbers of non-programming personnel concerned with data preparation, entry and retrieval throughout the sixties and seventies have an erratic presence within accounts of the development of computing.

[8] Cognitive science is a label that is applied to all disciplines which, in their different ways, aim to use a metaphor or model of the computer to understand, or construct, cognitive processes: the two most

to Japanese plans to develop a 'fifth generation' of computers and software built around artificial intelligence techniques, opened the door to cognitive science by formulating the 'Man-Machine Interface' in explicitly cognitive terms: for instance, it defined 'human/system cognitive compatibility' as a key area for investigation (Alvey, 1982: 29). The following year Card, Moran and Newell's seminal 'The Psychology of Human-Computer Interaction" (Card et al, 1983) was published, a work which conceptualizes the user as an information processor, where processing is expressed in cognitive scientific terms: for instance, it uses the idea of production rules as formulated in Newell's earlier work in artificial intelligence (Newell and Simon, 1972).

Therefore, whilst there have been, and continue to be, further developments in the area, it is in the early eighties that a distinct discipline begins to assert its identity and form a distinct discourse. Ergonomics and computer science continue to have an interest in the field, but the reading that I propose is that it is the impact of cognitive science, whether as cognitive psychology or as cognitive ergonomics, whose impact has been decisive. The relation between computer and user is seen as an essentially cognitive one, in that user behaviour is seen to be determined by the mental processes and representations (Long, 1989: 10); in cognitive ergonomics, there is the further implication that the development of knowledge about these mental processes should serve the purpose of supporting 'the optimization of human-computer interactions for effectiveness' (Long, 1989: 5). The stress on cognition and on effectiveness, though the latter is not always so directly expressed as here, exerts a powerful influence on the structure of the discipline. I explore the implications of formulating cognitive psychology/cognitive ergonomics as the core of the discipline below, but it should be noted here that whilst I wish to argue that its place at the centre has important effects on the discourse of HCI, it is not the case it has arrived in a new domain with a set of perfectly tailored methods, principles, findings or theories. Many of its practitioners see the domain as presenting entirely new problems which challenge cognitive psychology as it is currently constituted: the best discussion of these

---

explicitly cognitive scientific disciplines being artificial intelligence and cognitive psychology. For an overview of cognitive science, see Gardner (1985).

problems is Landauer (1988)[9]. However, little or no doubt is expressed by practitioners that cognitive psychology is better equipped than any other discipline to tackle the problems of human computer interaction.

Seen in this light, it is possible to specify HCI's relationship to computer science with a little more precision. Even though it is commonly identified as a core discipline, it is in important senses discursively constituted as external to the discipline of HCI. Many of HCI's personnel also work within mainstream computer science; however, within the theoretical framework set out in chapter two, I would argue that the biographical identity of authors who work in two fields is less significant than the different subject positions constructed for them within each discourse. For example, Long (1989) discusses the paradox that the work of two computer scientists on the development of a text editor in the late seventies (Bornat and Thimbleby, 1989) managed to exemplify aspects of cognitive ergonomic practice, and successfully address a number of design problems. Long wards off the possibly damaging implications that this work succeeds without recourse to any of the 'principles' of cognitive ergonomics, by arguing that their success is restricted to this particular software system, and, without the benefit of principles, is not generalisable (Long, 1989: 20). Thus the insights of computer scientists are marginalised with respect to the perceived requirements of HCI discourse. Both have continued to work within HCI; but they have had recourse to the emergent discourse of the discipline in order to legitimize their work.

If we accept then that biographical identity is beside the point, we must look for the basis of this externality in the discourses of HCI and computer science. The key feature is a perceived polarity between the purely technical concerns of a computer science which is concerned only with functionality, and the need to take into account the needs of the people using them. HCI

---

[9] Landauer also argues that as a consequence of the novelty of the domain, part of the interest of the area lies in its potential for developing psychological knowledge itself. I would say this is an (increasingly) minority view, and that the consensus is that the sole job of HCI is to produce implementable knowledge which can improve, in some way, software design.

discourse casts itself, with great unanimity and confidence, as the 'advocate[s] of the interests of users' (Landauer, 1988: 2)[10]. In this respect, it sees its task as trying to act upon and change computer science practice[11].

One consequence of this relation of externality, and indeed one reason for proposing it, is that HCI has a problematic relation to the actual practice of software design. The problem of how to get computer scientists, and especially software engineers to accomodate HCI considerations is a persistent theme. One approach is to formulate guidelines for designers, although these are mistrusted by many because of their extreme generality[12]. Another is to try to incorporate HCI' methodologies into the process of software development: for instance, by integrating task analysis and systems analysis (Sutcliffe, 1989), or by incorporating HCI concepts within Jackson's Design Methodology (JSD) (Lim et al, 1990). Probably the most significant aspect of this relationship is the apparent assumption that, since software engineers work with highly formal notations, HCI knowledge will have a better chance of making an impact if it is expressed as a

---

[10] Dijkstra's argument that computer science should aim to realize Leibniz's dream of providing symbolic calculation as an alternative to human reasoning is a particularly strong exemplification of the purely abstract, technical and functional approach of which HCI is so suspicious (Dijkstra, 1989): Winograd's response, which criticizes the complete lack of attention to the ways and situations in which computers are used exemplifies in turn the opposite pole (Winograd, 1989). In this case, at least, the suspicion is mutual judging from Dijkstra's disparaging reference to 'all soft sciences for which computing now acts as a sort of interdisciplinary haven' (Dijkstra, 1989: 1402).

[11] This technical/other (hard/soft) polarity can usefully be described (in technical terms) as a recursive structure, in that it is produced within HCI itself: for example, there is an increasingly large body of work which claims that more stress should be given to the understanding of user behaviour in context, and less to the formulation of cognitive processes within the user (for example Whiteside et al, 1988; Winograd and Flores, 1986): this is discussed in detail in chapter four.

[12] For examples of guidelines, see Shackel et al. (1988) and Brown (1988). For a critical view of this approach see Landauer (1988).

formalism[13]: hence, the proliferation of formal and semi-formal notations across a variety of approaches[14].

Thus, in terms of its rhetoric and its practice, HCI's self definition as a distinct field can be described as being built around a core of cognitive psychology and ergonomics, with computer science occupying a more ambiguous but in important senses external role. This constructed externality can be formulated as a necessity for the establishment of a separate discipline: if software engineers could improve the 'usability' of computer systems on their own, even with the help of guidelines, then the need for an active and separate discipline disappears.

Before looking more closely at some of the questions raised by multidisciplinary fields and their core disciplines, let us note two final points which are important for the understanding of the discipline. The first concerns its institutional structure. Within universities, HCI research and teaching is done within both psychology and computer science departments. However, a large part of its work takes place in industry. Shackel reports that 80% of the delegates at the CHI'85 conference in the USA were from industry (Shackel,1990: 30). In Great Britain the proportion is probably somewhat lower, although there is a good deal of joint industrial and academic research. On both sides of the Atlantic, there is a common aspiration, shared with most technology related fields, to be of value to industry; the relatively low proportion of industrial input being a frequent cause for regret to the British HCI community (see for example Sutcliffe and Macaulay, 1989: 5). This aspiration to industrial utility plays a significant role in the discourse of HCI.

The second and final point concerns the emergent status of the discipline. Whilst I argue that the structure of HCI discourse already defines many of the parameters of any potential contribution,

---

[13] This is a curious assumption in that it requires the software engineer to learn a new, and often impenetrably dense notation before he or she can understand what is being recommended.

[14] For example, GOMS analysis (Card et al, 1983), Payne and Green's task-action grammar (Payne and Green, 1986), and the design rationale approach (MacLean et al, 1990).

I also argue that the establishing of this discourse is still at a formative stage. Downes argued in 1987 that it was 'a field whose boundaries and central problem have yet to be defined', basing this assertion on the 'high scatter index' revealed by a citation analysis of one of its journals (Downes, 1987: 27). I would not fully concur with this, and in any case the field has moved on considerably in the intervening four years, but it emphasises that the discourse of HCI is still in the process of development[15]. Grudin's review of the development of the discipline reaches a similar conclusion (Grudin, 1990). I argue in chapter seven that some of the work of HCI practitioners can best be understood as part of the process whereby an emergent discourse attempts to solidify some of its objects and terminology[16].

3.2. Interdisciplinarity

To say that HCI is multidisciplinary is merely to assert the simultaneous presence of different disciplines. However, implicitly or explicitly, HCI's assertion of the necessity of a pluralistic approach suggests more than simply coexistence. It suggests the desirability of collaboration between contributing disciplines on a common problem or set of problems: in other words, interdisciplinarity. The very reasonableness of such an approach, implying as it does a receptiveness to ideas from other disciplines, inhibits criticism: and indeed, it is true that the theoretical sources that inform this thesis are themselves not drawn purely from within the discipline of sociology. Nevertheless, I contend that there are significant problems with interdisciplinarity which have been largely neglected within HCI and which are highly relevant to it: these problems are the subject of this section.

---

[15] I leave the question of the validity or otherwise of citation analysis to one side here.

[16] Fuller makes the suggestion that the early stages of disciplinary formation might be characterized not as the positive staking out of a domain but as a series of failures; the disciplinary field that is established is what remains after unsuccessful attempts to control other areas (Fuller, 1988: 192-3). Whilst in many respects this does not coincide with the account that I am proposing, it does offer an explanation for the unsettled state of a discourse in its formative stages.

Winograd's closing address to the CHI '90 conference on how computer science should be taught urges that the disciplinary base should be widened, but goes on to acknowledge that to view such education as 'interdisciplinary' is misleading. The problem, as he sees it, is that it is not feasible to attempt to turn computer students into 'amateur' social scientists and philosophers (Winograd, 1990: 445). This is a valuable objection in its emphasis on the autonomy of different disciplines, and a rare recognition of the difficulties of interdisciplinarity within HCI, but it is limited by its individualism; the problem is seen in terms of the individual's membership of one or another discipline, and by implication, of the amount of work that would be required to acquire competence in another. The crucial problems are better located at the level of the different discourses themselves.

Sharrock and Anderson take the whole question a step further by locating divergences between disciplines 'at the level of frameworks' (Sharrock and Anderson, 1987: 319). In the course of examining disputes between one form of discourse analysis practised by linguists, and conversation analysis, they argue that disagreement frequently focuses on specific issues and thereby erroneously assumes that both disciplines share the same frameworks of interest; whereas in fact, each discipline has its own set of questions and problems. The distinctive character of each is obscured in areas of apparent similarity. This is highly relevant for ethnomethodology in HCI, insofar as aspects of its terminology such as 'practical reasoning' can be taken to suggest, I believe incorrectly, that it has a theory of cognition which can be compared and contrasted to those of psychology. However, the problem of interdisciplinarity can be stated in even more fundamental terms if the issue is analysed from the epistemological standpoint that I have outlined.

Leps (1990) states that the epistemological model implied by interdisciplinarity is one of 'the subject adequately representing the separate object in language' (Leps, 1990: 285): in other words, interdisciplinarity is premised on realist assumptions. Woolgar spells out the implications even more succinctly in the course of an argument which confronts the apparent reasonableness of pluralism by a strategy of rhetorical inversion: 'My first objection to the call for pluralism is that it trades on the vicious imperialism of the triangulation myth. The notion of pluralism

implies that all contributors are contributing towards the study of a common object' (Woolgar, 1989: 217). The prior existence of a common object or objects is exactly what is denied within my theoretical framework, which, as we noted, views discourses as 'practices which systematically form the objects of which they speak' (Foucault, 1972: 49).

This is the basis of the problem, but in important respects it is not the problem itself. It seems to imply the impossibility of interdisciplinary collaboration, but as it stands it is an oversimplification. One legitimate objection is that intellectual currents cut across certain disciplines, with the effect that certain combinations may be more feasible than others. For instance, as Klein observes, the formulation that language constitutes reality, which informs this thesis, is now present within a number of disciplines within the humanities and social sciences (Klein, 1990: 268)[17]; collaboration here will clearly be easier than between, say, Wittgensteinian/Rylean philosophy of mind and cognitive science. Incompatibility therefore seems to be differential. A further objection, in relation to a new field such as HCI, is more fundamental. The constitutive character of a discourse may be granted, if it is simultaneously argued that in the act of collaboration a new discourse is formed which is derived from elements of all those that contribute and which can therefore constitute a new set of objects. In some respects this does describe the process that I have been proposing; the point of divergence is the assumption of equal contributions.

The key problem that I see with this notion of equal contribution is related to the existence of a core discipline within an interdisciplinary field. If it is accepted that the establishment of HCI as a distinctive discipline was largely the work of cognitive science, and in particular cognitive psychology, then the discourse of this discipline was from the outset derived from cognitive science discourse. Subsequent calls to widen the disciplinary base neglect the significance of this

---

[17] Consideration of the reflexive implications of this suggests a paradox: the epistemology which does most to cast doubt on the existence of objects antecedent to discourse, and therefore on the possibility of collaboration and interaction between different discourses, is itself located within a number of different discourses.

core discourse in setting the parameters for any contribution; Fuller argues that scientific work always operates within a 'possibility space' which is the result of previous work (Fuller, 1988: 244), an observation which is particularly pertinent to interdisciplinary work. The paradox of calls to widen the disciplinary base is that, for all their undoubted sincerity, the currently existing discursive parameters of the field may themselves prevent both the injection of fresh ideas that is sought, and therefore any genuine plurality. In sum, the problem for HCI, structured around a core discipline of cognitive science, and for would-be contributors such as sociology is the following: 'the imported product will always have the form of its appropriation rather than the form it exhibits "at home"'. It is 'brought into a practice .. in terms the practice recognizes' (Fish, cited in Messer-Davidow and Shumway, 1990:262-263).

The importance of this for sociology cannot be overstated, even though it has been largely neglected by contributors to date. It is for this reason that I now turn to examine some of the main features of the discourse of HCI as it currently stands.

3.3. HCI discourse

In referring to the 'main features' of HCI discourse, I am formulating a relatively broad approach to its analysis. Whereas Fuller emphasizes the primary importance of a discipline's 'argumentation format' for its assertion and demarcation (Fuller, 1988: 191), and Foucault, in his early work, studies the structure of the discursive field (Foucault, 1970; 1972), I intend to look at a number of aspects of the discourse: namely, at its objects, at some of its central themes, notable among these being representations of its purpose, at some of its rhetorical strategies, at some general parameters of its theoretical approaches, and at some of its own self-representations, for instance in terms of what might constitute agreed criteria for acceptable and legitimate work.

As will be apparent, these aspects of discourse inter-connect in a number of ways, and these connections are the chief reason why I have adopted a relatively broad analytic approach. In particular, I view the rhetoric of HCI discourse as central to the process of establishing its own terrain. In this I differ from a number of otherwise similar approaches. For instance, many

rhetoricians of science (see for example Keith, 1990: 365-366) see the analysis of rhetoric as essentially separate from questions of epistemic status[18]; similarly Lyotard (1984) argues that what he calls 'narrative', which is roughly equivalent to what I am calling rhetoric, forms a purely legitimating function in current scientific work. Whilst I agree that rhetoric serves a legitimating function, I argue that, at least in the case of HCI, such a function cannot be separated from the process of establishing a distinctive discourse, and that therefore it is tied up with the discipline's substantive, or non-narrative work. For example, rhetorical claims are a crucial part of the process of bidding for research funding; if funding is obtained, many of the parameters within which the research must be carried out may have been set by such claims.

The stance that I adopt towards the discourse of HCI involves playing off its assertions that the discipline has arisen in response to a set of already existing phenomena - to fulfil needs, to solve pressing problems - against the view of disciplinary formation that I have outlined: that is, a process of defining and asserting itself through the discursive production of phenomena, needs and problems.

3.3.1. Objects

Three objects which assume a pivotal role within HCI discourse are 'user interface', 'usability' and 'the user'; more than any other, they mark out the domain which HCI claims as its own. In this section I examine each in turn, and briefly consider a further object which has had less significance than one might have expected. To recapitulate, by 'object' I mean something which, within a realist epistemology, would be analogous to a concept. However, within the epistemology that I employ, it has a stronger sense: whereas 'concept' implies an attempt to represent already existing phenomena in discourse, 'object' as I am using it denotes the creation of phenomena by discourse. The significance of this position is particularly clear in the case of the user interface.

---

[18] This position is therefore perfectly compatible with a realist epistemology.

The user interface describes a locus of interaction between the computer and person. Its use within HCI discourse is ubiquitous, but attempts at precise definition run into difficulties. For example, the definition given earlier in the chapter - 'the medium through which a user communicates with a computer' (Benyon, Preece and Rogers, 1990: 13) - carries with it a number of connotations derived from the word 'communicates' which many would contest. In the majority of cases where it is used at present, it can be read as a description of both the visible representation of the software that is running which is displayed on the VDU screen, and the human actions that are needed to act upon the software.

Grudin (1990) points out that this conception of the user interface simply reflects the current state of computer technology. For example, at an earlier stage of development, interaction, such as it was, took place via punch cards and printout. He argues for a historical process in which the user interface moves steadily outwards from the electronic innards of the machine, through punch cards and printout, the VDU screen, the cognitive individual, finally repositioning itself within the social group or organization; the disciplines that concern themselves with this interface change correspondingly. It is in some ways a persuasive and subtle argument, but in terms of the relation of the discipline of HCI to its subject matter it exemplifies precisely what I have called the realist approach. For, despite his acknowledgement that it was in a particular period that the term acquired widespread currency, and that the predominant disciplines involved were cognitive psychology and human factors, he explicitly conceptualizes the user interface as existing prior to its formulation in discourse: 'Of course, systems have always had user interfaces: how have they evolved, prior to and since attracting attention?' (Grudin, 1990: 262). Or again, referring to the first generation of computers, 'This was not characterized as user interface development at the time, because when virtually all users were programmers the term "user interface" was not needed. (ibid: 263). The rhetorical effect of such assertions is that HCI, whose field is interface development, represents a necessary response to a given but hitherto unrecognized set of design problems, and that their solution has become a matter of some urgency as a result of the increasing numbers of non-programmers who now use computers.

If the user interface is the site of HCI practice, 'usability' is its objective. Despite its relatively early formulation[19], Shackel points out that 'there is, as yet, no generally agreed definition of usability and its measurement' (Shackel, 1990: 31). Its general sense - ease of use - is intuitively obvious, but the shades of meaning it acquires in different contexts vary considerably. A recent guide to usability states that it encompasses safety, effectiveness, efficiency and enjoyment (Benyon, Preece and Rogers, 1990: 14); the stress may fall on any of these factors in any given use of the term. Insofar as HCI has attempted to operationalise the concept in quantifiable terms as a means of evaluating systems, efficiency has been the relevant parameter. The classic, since most explicit, example of this is Card et al's keystroke model (Card et al, 1983), but it should be noted that the primacy of efficiency goes unchallenged even in overtly anti-mechanistic approaches towards usability evaluation such as Whiteside et al (1988).

There are two aspects of the term that can be highlighted. The first is that, like the notion of a user interface, its use generally presupposes that the functionality and (usually visible) form of a computer system can be unproblematically separated; this separation is certainly central to Bennett's (1984) seminal paper. The second is that, within this separate domain of form, usability is distinct from aesthetic considerations: the latter are seen as lying largely outside the concerns of HCI. Even where, as in Norman (1988), the necessity of a trade-off between usability and aesthetics is argued for, the aesthetic issues are left to one side.

In current usage, usability and user interface are closely related: to achieve the former being largely a question of redesigning the latter. However, many HCI practitioners would be reluctant to have their work restricted to interface design alone, although the extent to which they agree or disagree on this turns partly on the question of how broadly the interface is defined. Diaper, for example, argues that solutions in HCI need not always involve the interface, but may entail training or changing organizational practice (Diaper, 1989: 3). Furthermore, the separation of function and form that is implied in the use of both terms is frequently contested, for instance by

---

[19] Possibly the earliest use of the term in the general area of computer design was Miller (1971). However, Bennett (1979; 1984) is usually credited as the first to use the term in any systematic sense.

those who question the viability of attempting to improve the interface to a product whose functionality has already been defined, perhaps even implemented (see for example Draper and Norman, 1986: 2): usability on this account would have to include relating functionality to the tasks that users wish to perform. Nevertheless, when viewed in relation to current HCI practice, such assertions appear to be largely programmatic at present; and certainly, in terms of the perceptions of other disciplines and professions engaged in the general enterprise of software design and construction, this is the territory that HCI has staked out for itself[20]. This is a double-edged achievement. On the one hand, as Robinson (1990) argues, the separation of function and form necessarily assigns HCI a marginal position with respect to what systems actually do; but on the other, its formulation has helped HCI to establish its own domain within the greater enterprise of designing and constructing software systems.

Suppose that HCI were to be more fully integrated into the software development life-cycle, that is to say that it were to make a more significant contribution to the definition of system functionality: from where would it derive its legitimacy? HCI would still be able to claim a distinct role for itself with reference to a third discursive object, the user. One of the fundamental rationales for HCI's existence, which is omnipresent in its discourse, is that it, more than any other discipline, can adequately represent the user in the double sense that it can both understand him or her and can act as his or her representative. I examine HCI's representation of the user in more detail in the next sub-section, but it can be stressed here that HCI's conceptualization of the user is as a cognitive entity, and it is in this that its distinction lies, for the notion of user is not confined to HCI discourse but is employed throughout computer science to distinguish those who use software from those who write it[21]. Although HCI views the user in

---

[20] Again, representations and practice are closely inter-related. It is not simply that HCI's concern with usability is perceived by others to be confined to interface design questions, but that in actual system development, HCI personnel will in general not get their hands on a system until very late in the development process when the functionality has already been defined and implemented.

[21] The discussion in chapter five suggests some of the limitations of such a distinction.

cognitive terms, its conception tends to be equally generalised[22]. This is evident from the fact that the term is most frequently used in the singular, 'the user', and as such denotes a homogeneous constituency that, so goes the rhetoric, has been unrepresented for too long.

Finally, a discursive object which might be expected to be of fundamental importance is 'interaction', but as Payne (1990) has argued, despite its presence in the discipline's title it has been the subject of little attention. Most work has looked at either the computer or at cognitive aspects of the user, but not directly at their reciprocal relation, programmatic appeals notwithstanding (Barnard and Harrison 1989; Payne, 1990). I mention this because it can be read as supporting my position on the difficulties of interdisciplinary collaboration: for the computer and the cognitive individual lie within the remit of separate disciplines, and the inability to adequately formulate 'interaction' may stand for failed attempts at disciplinary interaction.

3.3.2. Representing the cognitive user

HCI's claim to represent the user implies both that it constructs accurate representations of the user as a cognitive entity and that it acts as the user's representative. The two senses are closely related in that the claimed legitimacy of the latter is partly derived from the former. The cognitive conception of the user has been one of the chief targets of ethnomethodological work within the field (Suchman, 1987; Gilbert, 1987), and other approaches which have sought to widen, or change, the theoretical basis for HCI work (Winograd and Flores, 1986); nevertheless, the agenda that cognitivism has set for the discipline still predominates, and, as I shall argue in the following chapter, some of its critics still operate within certain of its terms of reference.

---

[22] Attempts, within and outside HCI, to separate out 'types' of users are however equally problematic. For example, within the database literature rigid typologies are sometimes proposed, based on the kind of work that people do within an organization, and therefore the degree of access to the computer required and the nature of the tasks that they will need to carry out (see for example Everest, 1986). Within HCI there have been some attempts to classify people in terms of cognitive abilities and preferences: see for example the experiment that is the subject of discussion in Data 2.2, described in chapter 6.

The cognitive assumption can be explained on a number of levels. In its most general terms, it suggests that behaviour derives from, and can therefore be explained in terms of, inner mental activity. More specifically, such mental activity is expressed as 'mental representations and processes' (Long, 1989: 10). To the extent that psychological explanation works within the cognitive science computational model of cognition, processes can be viewed in terms of 'states' and 'state transition': Card, Moran and Newell (1983) typify this information processing model approach. To varying degrees, mental processes can be described as determinate: a particularly strong formulation being Long's assertion that part of the role of cognitive ergonomics is to specify 'the knowledge that determines user behaviour' (Long, 1989: 10). Finally, and following from the last point, cognitivist accounts describe human reasoning/action in highly rationalist terms as entailing the formation of plans and goals prior to action[23]; Suchman (1987) mounts a convincing critique of the credibility of such a hierarchical model.

The focus on mental representations is central to HCI discourse, and frequently expressed in terms of 'mental models'[24]. Norman describes mental models and their significance with clarity: 'I believe that people form internal, mental models of themselves and of the things and people with whom they interact. These models provide predictive and explanatory power for understanding the interaction' (Norman, 1986: 46). Representation within this broad paradigm therefore has an almost recursive structure, in that the task is to adequately represent a (hidden) representation that the user constructs of the software that is being used.

---

[23] A good example is Norman's 'seven stages of user activity' which involves establishing the goal, forming the intention, specifying the action sequence, executing the action, perceiving the system state, interpreting the state and finally evaluating the system state with respect to the goals and intention (Norman, 1986: 41). See also Storrs' discussion of agency, causation and interaction (1990: 324-326).

[24] The term originates in two books that appeared in the same year, Johnson-Laird (1983) and Gentner and Stevens (1983). However, Johnson-Laird's book, with its central stress on language comprehension, explored in terms of syllogistic reasoning, has had less impact within HCI.

Some of the consequences of this theoretical approach for HCI discourse and practice can be characterized in terms of indirection. HCI practice is indirect, or oblique with respect to cognition in two respects. Firstly, since cognitive processes and representations are inner, they have to be inferred. The experimental methods that prevail within cognitive psychology can be directly related to this requirement. In one method, the subject is encouraged, by formal or informal means, to speak during the course of the experiment, and their speech is used as a more or less direct index to their cognitive processes[25]. As I have argued, this is directly contrary to the stance that I take towards discourse. In another method, a mental representation is presumed and used as a causal variable in a controlled experiment. A good example of this is Kieras and Bovair (1984), where one group of subjects is confronted with a relatively abstract configuration of switches and asked to perform a sequence of actions to achieve certain effects, and another group is told that the configuration represents the console of the Star Ship Enterprise; results are compared, and findings are stated in terms of the role and efficacy of mental models. The central conceptual object which is presupposed in the experimental design again has to be inferred, this time from the subjects' performance[26].

The second aspect of this indirection can be expressed as an asymmetry between the object which forms the basis of cognitive psychology's claimed expertise, the cognitive individual, and the object that can be engineered, the computer. We may remember that one definition of the aim of HCI is that of 'designing humans and computers interacting to perform work effectively' (Long and Dowell, 1989: 14)[27]. Whilst the training of users falls within the scope of HCI - although practically it plays a marginal role - it cannot plausibly be described as a process of 'designing'. If the user is designed, this has to be done indirectly by changing the design of the software and thereby altering the parameters within which users work; again the associated change in the

---

[25] See for example, Carroll and Mack (1984).

[26] Garfinkel's formulation of the documentary method (see chapter 2) suggests that such inferential work is a feature of any theorizing, lay or professional. This is true, but arguably the inferential process is rarely so explicit as in the investigation of mental phenomena.

[27] See also Norman's use of the term 'cognitive engineering' (Norman, 1986).

user's knowledge is inferred from this. This indirect dimension contributes to what I described as the problematic relation of HCI discourse to the practice of software engineering.

The representation of the user as a cognitive entity therefore forms a central pillar of HCI discourse, and has a considerable effect on its methodology and practice. I now turn to the second sense in which HCI represents the user.

3.3.3. The user's representative: compassion and the performative

A pervasive feature of HCI discourse is its certainty that it can and does act as the user's representative. Its core discipline of cognitive psychology has an understanding of the user which a purely 'technical' discipline such as software engineering lacks. Beyond that, it is claimed that left to their own devices, software engineers do not consider the requirements of the user: HCI makes the (moral) claim that it should, and that it (HCI) can, through its articulation of users' needs and problems, change software engineering practice so that these needs and problems are taken into account.

The most explicit articulation of this position is the notion of 'user-centered design' (Norman and Draper, 1986). User-centred design should not only take the user into account, but should 'start with the needs of the user'; it emphasizes 'that the purpose of the system is to serve the user, not to use a specific technology, not to be an elegant piece of programming' (Norman 1986: 61). This illustrates well the terms of the overtly humanist and anti-technicist stance that typifies much HCI discourse, and which I will characterise as the rhetoric of compassion[28]. This rhetoric is evident in other forms. For example, the theme of the CHI '90 conference was 'Empowering People'. Another instance is the recurrent suggestion that designating software as 'tools' conveys, and hopefully leads to the realization of a conception of the computer as subordinate to the needs of the user (Norman, 1986; Forrester and Reason, 1990; Winograd and Flores, 1986).

---

[28] This term was suggested to me by Hugh Robinson.

There is a second dimension to HCI discourse however which appears to sit uneasily with this rhetoric of compassion, although I shall argue below that, viewed from a certain perspective, the two are integral to the process of disciplinary formation and mutually supportive: I call this dimension the performative. The term denotes two aspects of HCI discourse. Firstly, it suggests an orientation towards utility. HCI aspires to generate results which will be of practical benefit to the enterprise of system design; and the close ties with industry are highly relevant to this practical orientation[29]. Secondly, it carries the further meaning that HCI is concerned with the performance of the user: that is, with maximising efficiency in the work place. This has already been noted as an important aspect of definitions of 'usability'.

This concern with efficiency is a continuation of the concerns of ergonomics or human factors. Van Cott, in the course of a critique of ergonomics' unresponsiveness to human needs, defines that discipline as 'the first scientifically-based form of worker advocacy' but goes on to say that in practice it has 'extracted the performance required by the system' from the individual (Van Cott, 1985: 1136). Long's (1989) discussion of the role of cognitive ergonomics in HCI explicitly gives the production of 'desired performance' as the central goal towards which HCI should work. The concern with efficiency underlies, more or less explicitly, much HCI work[30]. The question to be asked is how these two rhetorics can co-exist within the same discourse?

---

[29] The interplay of the industrial marketplace and the academic domain, and its significance for the historical development of computer science and related disciplines is a complex and relatively unexplored issue. It is worth noting however Grudin's (1990) suggestion that interface design issues take on more importance in 'mature' markets: on this account, the market place creates some of the conditions of possibility for HCI's growth and self-assertion.

[30] Long's explicit articulation of efficiency as a defining feature of HCI makes two distinctions which are not typical however. He differentiates 'desired performance' or 'effectiveness' from speed (cf Card, Moran and Newell, 1983), and he goes beyond the generalised notion of 'the user' to stress that 'desired performance' is specified by the organization (Long, 1989).

One approach to this question that can be immediately dispelled is the idea that performative and compassionate rhetorics are different viewpoints held by two distinct sets of people within HCI. The two 'repertoires' (Gilbert and Mulkay, 1984) frequently co-exist within the same text, or even, as in the following extract from the opening address to the CHI '90 conference, within the same sentence: 'Let us then ask what we should do in tomorrow's user interfaces to increase productivity and empower people, as the theme of this conference charges' (Dertouzos, 1990: 1). Another striking example of such co-existence is Shackel's (1990) discussion of usability which argues for the importance of user-centred design, and goes on to discuss the impact on performance and productivity that attention to 'human factors' can have[31].

This apparent contradiction can be resolved by showing that, in many cases, user-centred design means no more than design which focuses 'from the start on users and tasks' (Shackel, 1990: 34). In this sense, empowerment means simply enabling the user to perform the tasks that are demanded of him or her: HCI's generalised notion of 'the user' and the associated lack of any distinction between the individual using the computer and the organizational constraints within such use takes place underpins the achievement of these highly restricted senses of liberal sounding rhetoric.

More fundamentally, the rhetoric of compassion can be directly related to the process of disciplinary self-assertion. If system design begins, as Norman argues it should, with the needs of the user, then HCI loses its relatively peripheral role in the software development life-cyle and moves centre stage as the user interface becomes central to the design process; in this respect, Norman's comment which follows this assertion 'from the point of view of the user, the interface *is* the system' (Norman, 1986: 61 original emphasis) makes the stakes clear.

---

[31] Again it is worth pointing out that the concern with efficient performance, and the perceived importance of HCI, could be related to the industrial market, since labour is now relatively more expensive than hardware or software.

This conception of disciplinary self assertion, derived from Foucault, may have another dimension. Whereas the process I have just described is one in which the discipline attempts to extend its role in relation to other disciplines, calls for user-centred design may also be read as effecting and legitimating its extension in its domain of study, computer use. Silverman's critique of the liberal rhetoric of 'patient-centred medicine' is suggestive here, arguing that such discourse serves to extend the gaze of the medical profession and open up more aspects of the patient's life to examination by the doctor (Silverman, 1987). Moreover, in 'Discipline and Punish' from which this model is largely derived, Foucault argues that the extension of the gaze of the human sciences in the penal domain is integral to a process of regulating, rationalizing and more efficiently administering punishment and detention, and subsequently many aspects of life in wider society (Foucault, 1977). In these terms, the rhetoric of compassion and the service of efficiency are part of the same process[32]. This has interesting implications for some critiques of overtly mechanistic approaches within HCI. For example, Whiteside et al's (1988) call for contextual approaches to usability research argues convincingly against some of the disadvantages of laboratory based usability evaluation methods, and advocates moving the research process into the user's natural work environment; but this could be construed, despite its compassionate rhetoric, as simply the next step in the extension of HCI practice into the domain of computer use.

I would not wish to overstress this last point, since some of its inevitable connotations of HCI as a regulatory force do not seem to adequately represent the general character of its practice; however, it does show that a discipline's rhetoric need not be taken at face value, and therefore that compassion and a drive towards efficiency are by no means incompatible. The most significant aspect of the rhetoric of compassion, and in particular the concept of user centred design, is that, whatever the intentions of those who speak and write it, it serves the process of disciplinary formation and extension in relation to other disciplines; and coupled with this, the performative dimension, in both of its senses, is a defining feature of HCI practice.

---

[32] I stress again that I am talking about the structure and effects of the discourse as a whole, and not questioning the sincerity of those who call for user centred design.

3.3.4. Self representations

(2.4: 3)

What I've done is, I've changed the notion of theory to knowledge. I mean I think we're not really interested in theories of HCI. I think the psychologists might be interested in a theory of HCI, where HCI was the phenomena which was the scope of the psychological theory, frankly I could be interested in that but I don't think HCI should be interested.

This statement was made by Long at the beginning of his contribution to a discussion at Interact '90 on 'New approaches to theory in HCI: how should we judge their acceptability?'. The basis for this strong proscription of theory appears to be derived from his explicit espousal of performative criteria that I have already noted. In his position paper to the debate, he states that any approach would be acceptable if it advanced knowledge which could effectively contribute to the solution of HCI's general problem of 'designing human interactions with computers for desired performance' (Long, 1990: 1057). Long's transformation of 'theory' to 'knowledge' reflects his view that any conceptualization must be capable of implementation: more explicitly, valid knowledge must be conceptualized, explicitly expressed in appropriate form or notation, operationalised, tested, validated and generalised (ibid: 1058). In the same vein, in the spoken presentation, he followed the opening statement that I have quoted by pointing out that he had, in his title, recast acceptability as effectiveness. Knowledge is acceptable if effective.

The subordination of theory to practicality is particularly strongly stated here, and there are certainly practitioners within HCI who would not subscribe to it. Nevertheless, examination of the field as a whole suggests that this may be quite representative, not in the sense that many others make similar statements, but in the sense that HCI is relatively unreflective. The problem is not that there is insufficient theory, but that the theoretical presuppositions which inform much of its discourse go unexamined. For example, the pervasive realism of work in the

field goes completely unremarked. Where theoretical reflection is explicit, it is either a reconceptualization of HCI which is directly oriented to improving HCI practice (Carroll and Campbell, 1989; Storrs, 1989; Forrester and Reason, 1989) or, less commonly, consideration of the question of HCI's disciplinary status: for example, as craft, engineering or applied science (Long, 1989; Long and Dowell, 1989). The former variant often consists of proposing unitary models of the phenomena of human computer interaction, with a view to correcting and clarifying existing misconceptions and thereby enabling better practice. Storrs argues that a single agreed model is essential if HCI is to make further progress in its practice (Storrs, 1989: 323); this perceived need for homogeneity is not one that is found in less explicitly practice oriented disciplines such as, for example, psychology. The general lack of theoretical reflection within the discipline reinforces the way in which the core discipline defines the domain of discourse, since it is from cognitive psychology that assumed and unstated theoretical presuppositions are largely derived[33].

A possible further consequence of this 'heads down' approach to its practice is that HCI remains, for all its multidisciplinary aspirations, relatively insular. Very few connections are made to work that goes on, and particularly debates that take place, in other design fields. For example, one of the few works from this general area which extends the discussion of design issues from the user interface to other artifacts is Norman (1988): he sees the central issue for user-centred design as the trade-off between usability and aesthetics, and the necessity of redressing the balance towards usability. This is a common theme within HCI. Yet even here, there is no acknowledgement of the exactly parallel, and relatively high profile debate that has been taking place in architecture. Another discipline which is in practice largely ignored is graphic design, despite claims for its inclusion within HCI (Winder, 1991). In this instance, insularity may be reinforced by HCI's scientific aspirations, discussed below, or to a perception of graphic

---

[33] It is worth pointing out however that, as Long's statement implies, explicit theorizing is by no means absent from cognitive psychology itself.

design as being too far towards the aesthetic end of the spectrum to be of value for HCI's work on usability. Either way, a potentially fruitful area of debate is left unexplored[34].

HCI aspires to achieve some form of scientific status, even though there is considerable disagreement over the precise details of this: Norman (1986) uses the term 'approximate science' whilst Long characterises the current status of much of its work as 'applied science' (Long, 1990). There is though a widespread if tacit consensus that rigour, which is desirable, can be equated with scientific credentials. For example, at the end of a paper which describes the design of one of the most innovative user interfaces to date, the authors state 'User-interface design is still an art, not a science. [..] Much of the time there is no scientific evidence to support one alternative over another, just intuition. [..] Perhaps by the end of the decade, user-interface design will be a more rigourous process' (Smith et al, 1982: 280). The apologetic tone here is striking. Furthermore, the design in question was the first explicitly graphical user interface, in which aesthetic considerations might have been expected to play a significant role[35]: in this context at least, one might expect that 'art' would be seen in less negative terms. The discussion of craft knowledge in Long (1989) gives some insight into this position. Craft knowledge is 'informal', therefore embodies no 'principles' and cannot be generalized, whereas both engineering and applied science knowledge provide principles to support practice (Long, 1989: 22). The conflation of formality and principle is noteworthy here.

---

[34] Hooper's (1986) exploration of the 'architectural analogy' is a notable exception here. A further dimension to this insularity was claimed by Downes (1987) who argued that there was relatively little internal debate and citation of others' work within the discipline as a whole. However, the citation analysis on which he based this conclusion was done four years ago, and the extent to which it still holds is uncertain: my impression is that there has been more discussion of other people's work in recent years.

[35] For instance, a study of perceptions of the Macintosh, which employed the same basic design strategy, suggests that aesthetic qualities constitute an important reason for the enthusiasm of many users (Jones, 1990: 24).

The aspiration to science can therefore be related to the desirability of establishing principles which can inform future designs. This is seen as a central problem for HCI by many. Landauer (1988) argues that the novelty of the domain invalidates the usefulness of much cognitive psychology, and that it does not have any principles that can be unproblematically applied; Long and Dowell (1989: 28) explain this in terms of the disjunction between explaining and predicting behaviour, which is the function of psychological principles, and the prescriptive principles that are needed for artifact design. The problem, widely recognized, is how to move from description to prescription. One might argue that HCI is haunted by the possible charge that the body of its work to date has consisted of attempting to explain why existing designs have been successful or unsuccessful, but has substantially failed to have an impact on the design process itself; countering such a charge is therefore frequently seen to require the establishment of a cumulative body of knowledge, on the basis of which principles can be specified which will inform and improve future design.

I would contend that this whole question is again underwritten by HCI's problematic relation to design practice: if other people, software engineers, are designing systems and using 'craft' knowledge to inform their designs, then there is no place for a separate discipline to specify what they should do; for craft skill is, by definition, something embodied in and learnt through practice[36]. The aspiration to science can therefore be seen as thoroughly implicated in the process of establishing HCI as a distinctive discipline[37].

---

[36] This is not to argue that software engineers would be any happier with the designation of "craft": indeed the claims that formal principles are a guarantee of rigour are even more strongly stated in this domain (Ince, 1988).

[37] The work of standards committees which is discussed in chapter 7 can be seen as a prime example of an attempt to establish principles for design.

3.4. Discussion: possible roles for sociology

I have attempted to show that although HCI is avowedly multidisciplinary, if it is examined as a discursive field many of the parameters within which potential contributions have to operate can be seen to be already in place. The significance of cognitive psychology as a core discipline has been indicated, and I have tried to show some of the most important features of its discourse as currently constituted: that it has a distinct set of objects; that it is demarcated from other disciplines, and therefore legitimated, by its cognitive representation of the user and its claims to act as the user's advocate; that it has a strong orientation to the performative, and that this has consequences for its self representation and its neglect of the significance of theoretical pre-suppositions. I want to finish by introducing the review of sociological work in the field. I shall do this by briefly considering the roles open to sociology in the field as it currently stands. Some of these themes will be taken up in more detail in the following chapter.

Recently, there has been a considerable increase in perspectives which take a more broadly social approach to the study of the user in context, and a rejection of some of the assumptions of cognitive science [38]. Grudin (1990) sees this widening of the theoretical framework to accomodate social concerns as a form of disciplinary progress (although it is also accounted for as a response to technological developments)[39]. However, I argue that the structure of the framework that I have described remains largely intact. I will illustrate this with three examples. Firstly, where there are calls to broaden the domain of study to take account of organizational issues, organizations are themselves conceived in cognitive terms as a collective process which comprises the formation of goals, plans, tasks and so forth (see for example Long, 1989; Eason, 1989). Secondly, recent (and belated) attention given to the significance of collaboration and interaction between individuals within organizations preserves the cognitive model by conceptualizing such processes as 'distributed cognition' (Hutchins, 1990; Hutchins and Clausen,

---

[38] See for example, Suchman (1987); Whiteside et al (1988); Winograd and Flores, 1986

[39] A less kind interpretation, following Fuller's (1988) suggestion that disciplinary formation is a process of failures to control phenomena in the domain, is that HCI is turning to other approaches after the failure of cognitive psychology to deliver the goods.

1991)[40]. Thirdly, the injunction to move the study of computer use out of the laboratory is framed within a set of explicitly realist assumptions. That is to say, although there are many points of contact with sociological approaches, the central emphasis on studying the user in his or her natural work environment (Whiteside et al, 1988) draws heavily on a rhetoric of the 'real world' which is at odds with some aspects of, for example, ethnomethodology.

Finally, I will suggest that sociological contributions to date can be characterized in the following way. The user, conceived as an individual, is taken as the given unit of analysis: that is, as a unitary subject who is the point of origin of her discourse. Closely related to this, whilst sociology disagrees with many aspects of cognitivist representations of the user, it shares many of its assumptions: its critique of cognitivism is built on the realist epistemology that underpins any attempt to provide a 'better' representation. Furthermore, it frequently utilises a modified cognitive framework itself in that it attempts to explicate reasoning, albeit with the aid of a different theory. It tends to adopt, or be uncritical of, some of the rhetoric of user-centred design. Finally, it is no coincidence that the variant of sociology that has perhaps made the largest contribution, conversation analysis, shares with HCI a commitment to 'construct a cumulative discipline' (Sharrock and Anderson, 1986: 80) based on a growing corpus of 'findings': as such, it is in a position to make a contribution to a discipline which, as we have seen, eschews work which fails to meet a set of explicitly performative criteria.

---

[40] This is not to deny however that 'distributed cognition' is innovative in relation to traditional cognitive psychology: in particular, in its conceptualization of cognition as distributed between not only individuals but also artifactual components of 'the system'. In this respect, it follows Norman's concept of 'knowledge in the world' (Norman, 1988). It may also be read as a cognitive parallel to Latour's arguments for the extension of the concept of 'the social' to artefacts (Latour, 1988a).

Chapter 4

Sociology: re-presentation and implementation

This chapter reviews sociological work in HCI to date, and relates it to what I have suggested are the main parameters of HCI discourse as it presently stands. It is a critical review, less in the sense of arguing the strengths and deficiencies of individual pieces of work than as a consideration of sociological contributions as a whole. I am interested in questions which centre on the relationship of this work to HCI: what does it have to offer, and what is it seen as having to offer? How is it that particular forms of sociology predominate? And what happens to sociology itself when it moves to a new discursive field? Following on from the discussion of the discipline of HCI in the last chapter, I argue that there are, in important respects, homologies between sociological work in this area and features of the discipline's core discourse which it is often seen as challenging.

Sociology in HCI can be characterised in terms of two main approaches. They are not mutually exclusive in that a given piece of work may contain elements of both. The first is to offer a new conception of the user and his or her understanding and utilization of a computer systems via sociologically informed empirical analyses of human computer interactions; that is, a re-presentation of the user. The second is to use sociology's existing knowledge about (human-human) interaction as a resource for engineering the interactional dynamics that computer systems enforce: this constitutes an attempt to implement sociological knowledge. These

approaches are used as organizing themes for this chapter. Critical questions about the nature of interdisciplinarity are raised in relation to the former, and about the epistemological status of sociological knowledge in relation to the latter; for to claim that it is suitable for implementation is to posit such knowledge as 'findings' in a very particular sense. A further question derives in part from perceptions within HCI of the importance given in sociological work to the issue of 'context'. Quite distinct work from sociology and related fields is commonly viewed, in an undifferentiated sense, as urging the inclusion of social context as a significant variable within any adequate account of human-computer interaction. The lack of distinction between quite different uses of the concept, the implicit empiricism of most appeals to context, and the idea that sociology can provide a contextually informed complement to existing work in the discipline are critically examined. The issue of realism is central to all of these questions.

The chapter is organised as follows. First, a descriptive overview of sociology in HCI is given. Then, the critical themes that I have outlined - conceptions of context, the re-presentation of the user, and the question of implementation - are examined in turn, making reference to such work as is relevant in the process.

4.1. An overview of sociological contributions

Within recent years the amount of work within HCI that could be classified as in some way taking a more 'social' analytic perspective on human-computer interaction has increased significantly, a development seen by some as a form of disciplinary progress (for example, Grudin, 1990). This chapter is chiefly concerned with work that can be unambiguously identified as at least drawing upon sociology, although some mention is made of other approaches which are perceived as similar in their emphasis on social aspects of computer use. The strand of sociology that predominates is undoubtedly conversation analysis (CA); indeed, all of the work described here can be categorised as CA, although there are important distinctions within this broad grouping. It should also be noted that, as might be expected, all this work can be described

as 'sociology for HCI'; that is, it aims to contribute to the discipline and to address the problems which the latter has identified as its own[1].

4.1.1. Conversation analysis

The seminal sociological work in HCI which draws on CA is Suchman (1987); however, given both its importance and some significant differences which distinguish it from other CA work, it is dealt with separately in the next section.

Readers unfamiliar with CA should be able to infer its salient features from the discussion that follows; a detailed introduction is therefore not given[2]. However, it may be useful to highlight a few points here. It is concerned to uncover the dynamic properties and systematic organization of conversation. Such organization is seen as the result of normative commitments and expectancies on the part of hearers and speakers, although it is frequently described in terms of systems of rules: precisely where the emphasis falls is subject to some variability. Finally, the question of how far CA is consonant with the aims of ethnomethodology, where its roots are generally seen to lie, is an open one; some work, for instance Suchman's (1987), can be clearly identified as ethnomethodological, other work less obviously so[3].

---

[1] The term 'sociology for HCI' is derived from Woolgar's (1985) contrast between 'sociology for AI' and 'sociology of AI', where the latter takes the discipline in question as the object of study instead of accepting that discipline's terms of reference. In these terms, this thesis, unlike the material reviewed here, is explicitly a sociology *of* HCI. However, as the two categories are not necessarily disjoint, this does not preclude the possibility of making a recognisable contribution to HCI, although the status of the account will in this respect be largely dependent on the extent to which HCI is willing to grant self-reflection as a legitimate feature of its own discourse. This question is addressed in the final chapter.

[2] Drew (1990) provides a particularly useful and succinct account of the significant features of CA.

[3] One area of difference would be where (syntactic) structure is stressed at the expense of normative expectancies and order as a practical accomplishment. Another concerns CA's aspirations towards realism: certainly, the stated aim of its founder, Sacks, to establish a natural observational discipline

Two projects at Surrey University attempt to incorporate CA formulations of the dynamic and orderly properties of conversational interaction into the design of user interfaces. In the Advice System (Gilbert, 1987; Frohlich, 1988; Frohlich and Luff, 1989; Frohlich and Luff, 1990), the dynamic properties of a direct manipulation interface are based on models of conversational structure: turn-taking, repair, openings, continuations and closings are all implemented as dialogue control policies. For example, the user can be selected as the next 'speaker' or can self-select; or again, 'side-sequences' are provided so that the user, and the system, can repair misunderstandings via the provision of meta-questions which can be used at appropriate places in the dialogue. It should be noted that in implementing 'the technology of conversation' (Sacks, 1984: 13) on a type of interface which is in many respects not obviously dialogic, Frohlich and Luff are implicitly making a very strong claim about the applicability of CA to interaction in general.

In the second project, conversation analytic insights are applied to the structuring of 'voice-mediated human-computer dialogues, where the user speaks to the machine and the machine's responses are generated using a speech synthesiser' (Gilbert et al, 1990: 236). Like the Advice System, it is in relation to the system's management of dialogue that the implementation of CA findings is attempted. Postulated features of conversation such as adjacency pairs are represented as grammatical formalisms suitable for interpretation by a parser or a generator; that is, computational models of conversational phenomena are being produced. In doing this, Gilbert et al explicitly question claims that computational linguistics and CA cannot be, at least partially, integrated (1990: 256).

Cawsey (1990) also works to integrate CA with computational linguistic approaches. However, there is a significant difference of emphasis. Part of the rationale for the employment of conversation analysis in the two projects outlined above is that it obviates the need to solve the

---

(Schegloff, 1989: 202) places it at odds with reflexive readings of ethnomethodology. However, as was noted in chapter two, this latter tension runs through all forms of ethnomethodology.

intractable problem of how to model the user (Frohlich, 1988); the CA stance being that interaction is locally managed in terms of speakers' orientations to, for example, the preceding turn, rather than by the inference of the other speaker's cognitive state or intention (Gilbert, 1987). By contrast, Cawsey uses CA to complement a plan-based approach to the generation of explanations by an expert system, an approach which attempts to model the user in terms of what he or she needs to know at a given point in time. CA here is used to improve the interactive qualities of such explanatory processes, giving the user the opportunity 'to interrupt and redirect the explanation' (ibid: 222). Cawsey argues for the necessity of this combination of local management and cognitive modelling.

The relevance of CA for the repair of misunderstandings is an implicit or explicit claim of all the above work. It forms the sole focus of a number of other papers. McTear (1985) argues that work within natural language processing could extend the scope of CA insights into repair mechanisms within the domain of human-computer interaction, where turn-taking is necessarily rudimentary in comparison with the subtleties of 'naturally occurring conversation'. It is argued that the specifically 'transactional' nature of human-computer interactions implies the need for the system to make inferences about the user in the absence of the shared knowledge that is a feature of social interaction. Again, a form of synthesis is urged. Raudaskowski (1990) produces a prototypical implementation of CA repair strategies in a telephone message system, using a human intermediary. This is used as the basis for an experiment which attempts to evaluate the effectiveness of these strategies.

This brings us to the use of CA as a means of evaluating interactions with computers. Of course, this is not necessarily disjoint with respect to system design, since results can be fed back into the design process in the form of recommended modifications. Norman and Thomas (1990) suggest that CA can be understood as relevant to interaction in general, and use it to analyse 'users interacting with a variety of interactive systems in unconstrained settings' (ibid: 59). The result of such evaluation is a set of design guidelines which are informed by both the findings and the methodology of CA; consequently, they claim, these guidelines will be 'principled' in a way that others are not.

McIlvenny's (1990) wide ranging discussion draws on the interpretative resources of CA in the analysis of a number of interactions; however, his work is comparatively unusual in that it keeps the question of CA's suitability for this domain open whilst simultaneously exploring its possibilities. Like Suchman, the focal point is the user's practical reasoning, and the insights of CA are used primarily to inform that broadly ethnomethodological enterprise rather than to provide a descriptive model of the structural dynamics of the interaction[4]. The particular experiment reported in fact concerns an interaction via a computer 'phone'; in other words, a computer-mediated interaction. Some of the peculiarities of this particular modality are studied in terms of their effects on the participants' mutual understandings, and their capacity to reveal the 'normal flow' (ibid: 114) that has been disrupted. The research is seen as a step towards the general goal of 're-embodying' CA by implementing it in interactive artifacts.

A final, and in some respects marginal approach to the use of CA in HCI is provided by Finkelstein and Fuks (1990)[5]. They seek to understand the process of software specification, and ultimately support it with computer based tools; the interactions between people involved in specification are examined in conversation analytic terms[6].

4.1.2. Suchman

Suchman's (1987) study of the problems faced by users attempting to make sense of a newly designed expert help system on a photocopier is the most impressive piece of sociologically

---

[4] McIlvenny differs from Suchman however in the emphasis he places on the embodied nature of action and reasoning, and in the significance of physical presence for communication.

[5] Although this is the last piece of work mentioned here, it should be stressed that other work within HCI does make reference to, or even makes some use of, CA: for example, Payne (1990), and Forrester and Reason (1990). I have concentrated on materials in which CA has a more than incidental presence.

[6] It is questionable whether such work falls within HCI as it is presently constituted. However, it should be noted that the discipline pays increasing attention to supporting the design process (see chapters 6 and 8 below); on this account, Finkelstein and Fuks' work is not untypical.

informed work in HCI to date, both in terms of its empirical detail and its theoretical sophistication; it has also had by far the most impact, and was, in the first instance, the inspiration for the research reported in this thesis. It comprises a detailed articulation of the insights of ethnomethodology and CA, an argument for their relevance to theories of reasoning and action, and a semi-formal experimental study in which pairs of users are videotaped attempting to carry out specified tasks on the photocopier.

Suchman argues that the difficulties users encounter are closely related to the theory of human cognition which has informed the system design. The theory in question is explicitly rationalistic in that it conceives the reasoning process to be one in which plans are drawn up prior to, and thereafter determine the course of, action; accordingly, a given action can be read as the instantiation of part of a plan. This assumption is explicitly incorporated into the help system which attempts to infer the user's current understanding, and therefore the information he or she requires, by locating his or her most recent actions within a specified sequence which is mapped onto a corresponding schema of goals and sub-goals. Suchman's empirical materials, however, demonstrate that the system fails to support the user's action and understanding as hoped; she suggests that the frequent misunderstandings that occur can be explained in terms of the deficiency of the planning model.

Suchman proposes a different model. Action is better understood as 'situated', that is, its organization 'is an emergent property of moment-to-moment interactions between actors, and between actors and the environments of their action' (Suchman, 1987: 179). Plans do not have a determinate relation to action, but are rather ways of accounting for it by formulating its 'antecedent conditions and consequences' (ibid: 3): as such they form, at best, resources for action[7]. Suchman's transformation of the cognitive science concept of plans and actions is two-fold: for not only is action related to the immediate circumstances in which it takes place, but plans are

---

[7] The indeterminacy of plans suggested here can also be explained with reference to a Wittgensteinian view of rules: since a rule, or plan, can never fully explicate its mode of application, it cannot determine the action that it attempts to prescribe.

reformulated as constructions that emerge in the course of social interaction instead of internal cognitive entities[8].

CA forms an indispensable resource for Suchman's work, but whilst it is clearly ethnomethodological, there is good reason to avoid characterising it *as* CA. Or rather, there are important distinctions to be drawn between Suchman's analysis and other CA work in HCI[9]. The key feature in this respect is her interest in the substance of accounts as an analytic resource. Whilst it would be caricatural to suggest either that CA has no interest in this, or that Suchman has no interest in the dynamic structure of the interactions she studies, a clear divergence of emphasis is undeniable. Furthermore, there is no attempt to describe the interaction with the artifact in conversation analytic terms; she argues that 'communication' between participants and artifact is severely impoverished in comparison with communication between participants themselves, and devises a special transcription layout to demonstrate this. Thus whilst the insights of CA are crucial to her analysis, she avoids some of the pitfalls of assuming that conversation is a suitable model for interactions with computers[10].

Suchman's analysis is premised on the following: 'through the ways in which each collaborator works to provide her sense of what is going on to the other, she provides that sense to the researcher as well' (ibid: 115), speech being the 'richest source of information for the researcher' (ibid: 116). Her interest in accounts is therefore that they make available the user's practical

---

8 Whilst this reformulation of the relation of accounts to action is, as she argues, generally consonant with a lot of work in ethnomethodology, there is a specific and unreferenced antecedent for her argument in Mills (1940), which argues that 'vocabularies of motive' should be understood as 'situated actions'.

9 The issue here is not one of pedantic niceties of categorisation, since there is in any case no definitive boundary between ethnomethodology and CA: but it is important to demonstrate a significant difference of emphasis between Suchman and the work described in the previous section (with the possible exception of McIlvenny).

10 Agre dissents from the view put forward here: 'for Suchman [..] the prototype of all action is conversational interaction' (Agre, 1990: 371).

reasoning. There are certain problems with this in the context of her argument to which I will return in section four.

Finally, let us note that whilst Suchman's analysis addresses, on one level, the problems associated with a specific system and goes so far as to make a number of, in my view unsatisfactory design recommendations, the chief interest of her argument is the alternative view of human action and reasoning that it proposes; certainly, its impact within HCI derives largely from this. Suchman concludes by pointing out that understanding the limits of machine behaviour and attempting to improve artifact design 'could contribute much to an account of situated human action and shared understanding' (ibid: 189). Her work is therefore not directed solely towards implementation issues; in this it is comparatively unusual[11].

4.1.3. Other 'social' perspectives

There are a number of other approaches within HCI which are widely perceived to be broadly similar in intent and implication to some of the sociological work described above. Whilst this perception neglects some fundamental differences of emphasis, there do share some common ground. This can be defined as a concern with setting or context, and a critical attitude towards conventional theories of cognition; the two being, in most cases, mutually dependent in that the latter are seen as positing cognition as a purely internal process and neglecting the significance of its interplay with the settings in which it takes place[12].

Winograd and Flores (1986) draw upon the philosophies of both Heidegger and speech act theory to mount a critique of cognitive science[13]. The latter's model of reasoning is seen as being

---

[11] Landauer (1988) makes a similar point in relation to the possibilities that the study of human-computer interaction hold for the development of psychological knowledge.

[12] Lave argues however that the separation of cognition and practical action in social settings is a limitation not only of cognitive psychology but also of much anthropology (Lave, 1988: ch 1).

[13] This is an uncomfortable theoretical synthesis. A central incompatibility derives from the significance given by speech act theory to the speaker's intentions in its interpretation of language use. Heidegger, by

incorrectly premised on the primacy of conscious and detached reflection; they argue, by contrast, that reasoning is embedded within social and linguistic settings and backgrounds of pre-understanding, and engaged and non-reflective during the normal course of practical activity. They suggest that such an approach, challenging as it does the prevailing orthodoxy on the user's mental representations, has implications for system design, although for all their theoretical eclecticism, the substantive content of their recommendations is largely derived from specific aspects of speech act theory.

Activity theory (Bodker, 1989; Bannon and Bodker, 1991) draws on a different set of theoretical resources - primarily the Soviet social psychology of Leontiev and Vygotsky - to formulate a broadly similar position. Current approaches towards interface design are seen as constrained by an overtly abstract conception of the user and his or her cognition. Instead, human-computer interaction must be related to the (work) activities of which it is a part; an adequate assessment cannot be based on the consideration of either design or cognition in isolation from such activities. Grudin (1989) makes a similar point in relation to the formulation of abstract design principles such as 'consistency'.

A related emphasis is discernible in recent moves towards the theorization of distributed cognition. Here, the cognitive science focus on cognition within the individual is not so much challenged as widened. Norman (1988) is seminal in this, distinguishing 'cognition in the head' and 'cognition in the world'; it is developed more explicitly in Hutchins and Clausen (1991). The distinctive emphasis here seems to be the conceptualization of people and the artifacts that they use as collectively forming cognitive systems.

Agre and Shrager (1990) provide an analysis of skill acquisition in a 'microgenetic' study of someone engaged in carrying out a routine photocopying job. This analysis is unusual in that it

---

contrast, explicitly challenges this emphasis on subjectivity in philosophy. On this topic see Derrida's 'Speech, Event, Context' in Derrida (1982).

comprises an extremely detailed account of solitary, silent, embodied activity as recorded on video-tape, and stresses the 'dialectical relationship between the person-in-activity and the setting in which the activity takes place' (ibid: 694). It has something in common with some of the ethnomethodological 'studies of work', in particular Sudnow's (1978) detailed account of his own acquisition of embodied competences whilst learning to play jazz piano, but it remains unique within the cognitive science literature.

Contextual research (Whiteside et al, 1988), like Winograd and Flores, makes explicit use of Heideggerian ideas, but the central emphasis here is a methodological injunction to move the evaluation from the laboratory into "the user's natural work context" (Whiteside et al, 1988:806). It is argued that such context sensitive research has the advantage of letting the criteria for usability emerge from the user's experience, rather than imposing criteria on him or her in, for instance, a benchmark test[14]. Of course, there are methodological implications in any theorization of the significance of setting for cognitive phenomena; however, contextual research differs from the other approaches outlined here in the extent to which it places questions of methodological adequacy at the centre of its concerns. The significance of differences such as this is the focus of the next section.

4.2. The question of context

There is a tendency within HCI to group together much of the work discussed in the last section, in terms of the common concern with 'context'. For example, Carroll and Campbell (1989) represent Winograd and Flores (1986), Suchman (1987), and Whiteside and Wixon (1987) as sharing the view that "artifacts cannot be understood apart from the situations in which they

---

[14] Lave's critique of laboratory based experiments in cognition makes the similar point that if a problem is specified for the subjects, no matter how informally, the research design misses out on the ways in which people formulate as well as solve problems (Lave 1988).

are used" (Carroll and Campbell 1989:251), and that the effects of particular design features on usability are only discernible in the empirical settings in which they are used. Without wishing to dispute this statement or dissent from its message, there are important differences of emphasis between these approaches which need clarification. On one level the distinctions are subtle; but on another they are important, since their neglect can only result in an impoverished conception of the nature and significance of context within accounts of human-computer interaction. I begin by identifying different uses of the concept.

Suchman (1987), as we have seen, argues that the immediate situation provides a set of resources which users draw upon in a moment-by-moment fashion in their interpretation and use of computers. The significance of context here is therefore that it forms an indispensable resource for the user's practical reasoning. However, this has to be qualified given Suchman's use of conversation analysis; for CA tends to restrict the notion of context to features of conversation itself, such as the preceding turn. It can even be seen as engaging in the formulation of "transcontextual" features of interaction (Drew 1990:27), that is, universal features which operate regardless of setting[15]. Indeed Frohlich and Luff explicitly, and favourably, contrast its 'context-independent' qualities with the 'context-dependence' of psychological approaches to the understanding of dialogue (Frohlich and Luff, 1990: 197). Accordingly, it is the temporal or sequential context which is most significant for Suchman, rather than any setting external to the details of the interaction. For example, her study uses a laboratory experiment, albeit an informal one, and does not deal specifically with the issues of how far its findings could be

---

[15] I am considerably oversimplifying a complex question here. For instance, Sacks, Jefferson and Schegloff's seminal paper suggests that the organization of turns can be seen as "context-free and also capable of extraordinary context sensitivity" (Sacks et al, 1978: 9). It is also true that conversation analysis does take an interest in talk in specific settings, for example in law courts (Atkinson and Drew, 1979). In general however, there is a reluctance to posit such settings as explanatory features for the organization of conversational interaction, but rather an emphasis on the way such organization is a constitutive feature of those settings.

generalised to other settings.

In Winograd and Flores (1986) the importance of context is again formulated in relation to users' reasoning processes, and in opposition to a pervasive form of rationalist account, but in different terms. Whereas Suchman formulates context as a resource for the user to draw upon, Winograd and Flores lay emphasis on the setting, broadly conceived as a hermeneutic horizon of pre-understandings and practical activity, as that which both shapes interpretation and gives meaning to action; in an important sense, action and setting are inseparable.

The contextual approach to usability evaluation (Whiteside et al, 1988) is primarily concerned with questions of methodological adequacy, going so far as to claim that the use of this approach and its associated techniques, such as the "contextual interview", guarantees more direct access to the user's ongoing experience. We shall address the shortcomings of such claims shortly.

One further variant on the formulation of context can be seen in Grudin's (1989) argument that designers should pay more attention to the context of use instead of letting their design be guided by more abstract principles such as consistency. Here context is recommended as a counterweight to form and formalisms in design practice.

This is not of course an exhaustive summary of the ways in which the notion of context is utilised in HCI, but rather a brief sketch which is intended to show that the concept can be enrolled in the service of quite different causes. Suchman describes a temporal context and argues that it forms an indispensable resource for the way users reason, and uses an laboratory setting and loosely defined experiment to support this claim; Winograd and Flores conceive the setting altogether more broadly as a linguistic and social environment from which action and interpretation cannot be separated; the contextual approach to usability evaluation is concerned with obtaining more accurate representations of users' experience, and argues that studying them in their normal environment and minimizing some of the potentially distorting effects of the researcher's presence and intervention will greatly assist such accuracy; whilst Grudin argues

that paying attention to the ways in which systems are used will offset the tendency to use abstract design criteria, and thereby result in better systems.

There are significant differences here, most obviously between Suchman and contextual research, differences which are consistently elided in commentaries. Kellogg for example, sees Suchman's work as a contribution to the general task of analysing '*real people in real situations*' (Kellogg, 1990: 193, original emphasis), her use of laboratory experiments notwithstanding. Carroll articulates the challenge of this kind of work in terms of its perceived implication that system design must somehow try to accomodate the infinite details that emerge from an artifacts use *in situ*, and that can have substantial effects on its usability (Carroll, 1990b: 322). Here context is not only a question of 'real' situations of use, but is conceived as posing a problem of quantitative detail.

The difficulty with these statements is that they completely neglect central issues such as selection, interpretation, assessment, relevance and representation; the effect is to generalise the whole question of "context" in such a way that it suggests no more than a particular research method which begins from the "bottom-up" study of empirical facts in particular settings. This neglect of questions of interpretation results in a form of realism in which context is taken as an empirical given which is available for inspection by the analyst.

One consequence of this is that the primary significance of sociology, and related perspectives, is perceived in terms of its attention to the empirical. As such, it can be easily assimilated within HCI as a complement to existing work. Even sympathetic commentators see it in these terms: Robinson, for example, suggests that ethnomethodology 'offers a way of restoring context and situation to the descriptions and accounts of human-computer interaction' (Robinson, 1990: 45) and a way of 'assessing empirical claims in HCI' (ibid: 48). The distinctiveness of sociological discourse is ignored on such a view; the ways in which sociology, and particularly ethnomethodology, might *formulate* context are left to one side. A more extreme version of this approach makes the stakes even clearer: Barnard and Harrison (1989), reduce contextual issues to variables which can be accommodated within more orthodox approaches to building predictive

cognitive and system models of interaction; yet Suchman's work, which they cite, stands as a challenge to precisely this kind of predictive approach.

However, it is not entirely fair to say that sociology has been misunderstood in this respect, since too often it too subscribes to realist formulations of context of this sort. In chapter five, I outline a different approach to the interpretation of context, in the course of analysing empirical materials which differs in two respects from the received view against which I have argued. Briefly, context is analysed as a social construct which is created in the course of conversation between a computer user and her colleague; the empirical details of the setting have a contingent relation to this context. Furthermore, the formulation of context is subject to interpretative flexibility no less by researcher than participants: it is therefore constructed in a double sense. On this account, sociology's perceived role of providing a contextual complement to other work in HCI, via close attention to hitherto ignored empirical details, is thrown into question; for not only is context formulated as a matter for interpretation, but in a more fundamental sense it is regarded as a discursively created object. To assume that sociological accounts of context could be grafted onto descriptions of interactions that originate in different theoretical traditions is to underestimate the creative significance of discourses for their objects of study, and thereby to fail to recognize the theoretical complexities of interdisciplinary collaboration.

4.3. Re-presenting the user: homologies and shared assumptions

We have seen that the extent to which sociology is enrolled as a resource for the criticism of cognitive theories is variable; sociological accounts may be presented as explicitly critical (Suchman, 1987; McIlvenny, 1990), as complementary (Gilbert et al, 1990), or, more equivocally, as offering alternative strategies to the solution of currently intractable problems (Frohlich, 1988; Gilbert, 1987). Most sociological accounts in this domain however purport to offer different representations of the user and his or her interaction with computers, whether such difference is expressed as criticism or alternative. I hope to show however that, in important respects, sociology in HCI can be seen to share many of the central features of what I have called the core discourse of cognitive psychology. I take Suchman's work as the main focus for this discussion

because it is the most explicitly critical, and because it articulates a theoretical position in relation to cognitivism which is at best implicit in other sociological work.

It is notable that sociological criticism of cognitive theory within HCI tends to replicate the stance that HCI in general takes towards computer science and software engineering. HCI advocates a user-centred approach to system design which takes account of more than simply 'technical' factors. Sociological perspectives within HCI suggest that the user be represented in terms which recognize their distinctively human qualities, and not as, for example information processing entities. For example, Suchman notes that whilst the planning model that she challenges is deeply ingrained in Western thought, its current predominance can be related to the fact that it is 'attractive for the purpose of constructing a computational model of action' (Suchman, 1987: ix), but that this is inadequate for describing the 'essential nature' of human action (ibid: x). McIlvenny is similarly critical of accounts which view human communication 'as the abstract interchange of symbols between individual cognitive processors' (McIlvenny, 1990: 93). Not only is the task of representing the user paramount then, it is necessary that his or her distinctively human qualities be recognized. The further claim, implicit in much of this work, is that sociology is well placed to provide such a human-centred account.

There are a number of points to be made about this stance. In the first place, it is suggestive of the extent to which sociology has been concerned to contribute to HCI, and has not, until now, taken an interest in HCI as a topic. Thus for example, the 'rhetoric of compassion' and its entanglement with questions of efficiency and performance has never been challenged; the discipline's goals have been taken as given. Insofar as sociology challenges work in HCI, it is by way of claiming to provide better representations. Suchman is aware of the stakes here: she recognizes and affirms the SSK injunction 'to eschew interest in the validity of the products of science, in favor of an interest in their production', but admits that her work is unashamedly interested in that she is committed to an alternative account (Suchman, 1987: x).

This brings us again to the issue of realism. Underlying Suchman's commitment to a sociological description is an explicitly realist epistemology; for she 'take[s] it that there is a reality of

human action, beyond either the cognitive science models or my own accounts, to which both are trying to do justice' (ibid: x). Woolgar (1987) has already discussed this in detail, arguing that critics of cognitivism, including Suchman, 'reproduce an impoverished version of what they are actually fighting against' as long as they remain 'committed to an ideology of representation' (ibid: 326). That is, they replace the reduction of behaviour to cognitive explanans with its reduction to social explanans: 'what seems cognitive is actually social' (ibid: 316). The commitment to realism is therefore a shared one.

There is a further aspect to the relationship between the discourses of HCI and ethnomethodology in this respect. Whilst most work on the social sciences is realist to an extent in terms of its aspiration to adequately represent people, and more generally phenomena, claims for representational adequacy are frequently seen as fundamental to the two discourses in question, and particularly in relation to their respective disciplinary alignments. For just as HCI's legitimacy is founded on its representation of the user to, and within, computer science more generally, ethnomethodology's distinctive contribution to sociology is often posed in terms of the confidence with which it explicates the social 'member' and his or her world[16]. They are in this respect mirror images of each other.

Let us move on to consider the content of these representations in a little more detail. Woolgar's discussion clearly illuminates the underlying realism of critiques of cognitive theory: however, in characterizing Suchman's work as arguing that 'what seems cognitive is actually social', he implicitly accepts the proclaimed status of her account as criticism delivered from the standpoint of a fundamentally different understanding of action. By contrast I will argue that, in significant ways, her account shares other features in common with cognitive theory beyond

---

[16] This was discussed in chapter two. Frohlich and Luff provide a good example of how this confidence about the representation of the member translates into confidence about the representation of the user when they state that, of all the possible approaches to dialogue control policy, 'a CA approach is most likely to fit the expectations about the meaning and structure of conversational behaviour which users themselves bring to the system' (Frohlich and Luff, 1990: 194).

epistemological realism. These all relate to the ways in which discourse is used as an empirical resource.

Suchman's method is to organise subjects into pairs, since in the interests of collaboration 'each makes available to the other what she believes to be going on'; in so doing, 'she provides that sense to the researcher as well. An artifact of such a collaboration, therefore, is naturally generated protocol' (Suchman, 1987: 115). Of this protocol, 'the richest source of information' is the verbal protocol: 'In reading the instructions aloud, the user locates the problem that she is working on. Her questions about the instructions identify the problem more particularly, and further talk provides her interpretations of the machine's behaviour, and clarifies her actions in response' (ibid: 116). My reading of this is that, just as in much cognitive psychology, accounts are being used as the basis for inferences about the speaker's thoughts and reasoning processes; the underlying model being that speech is the, more or less direct, expression of thought. It is therefore itself an account of cognition, and is critical of cognitive theories only to the extent that it proposes a different model of the ways in which people reason.

Suchman's account is not therefore one which attempts to replace the cognitive with the social; in fact, the significance of social interaction for the researcher's interpretation of discourse is explicitly restricted, as we shall see below. But let me first note that Suchman's work has indeed been interpreted within HCI and cognitive science as a novel theory of cognition. The received view is that Suchman is simply suggesting that cognition is more reactive and improvised than more rationalist approaches suggest. Kellogg for example, locates her work within a shift towards the understanding of 'embodied cognition' (Kellogg, 1990: 193), and even as a contribution to the 'psychology of human-computer interaction' (ibid: 197); whilst Agre suggests that one of her main insights is 'the idea that cognition might be something that takes place in the world' (Agre (1990: 378)[17]. In the sense that ethnomethodology can be taken as providing

---

[17] The recent move within cognitive science towards locating cognition in the world , as alluded to in section 4.2.3., seems to me to preserve the category of cognition whilst making concessions towards the

accounts of people's reasoning, Suchman's work merely applies this approach to the study of a new domain. However, in that new domain it perhaps takes on connotations which were not readily apparent within sociology; or to reformulate this, it highlights an area of interpretative flexibility within ethnomethodology which was neither so apparent nor so important before it began to interact with the discourse of cognitive psychology. The key issue, as Lemert identified, is that without more explicit attention to the role of language (and therefore of social interaction) and its relation to the speaking self, ethnomethodology remains 'a sort of social psychology' (Lemert, 1979: 303); that is, it studies the reasoning of the individual in the context of his or her social circumstances. This seems to me to describe well both the main thrust of Suchman's account, and the way in which it has been received. Social interaction is effectively assigned a merely mediative role.

I have suggested that this central aspect of Suchman's account can be partly related to tendencies within ethnomethodology and to the unpredictable connotations which can result from the move to a new discursive field. But the issue can be approached on a different level by looking at the way in which her account explicitly marginalises the social. Justifying her use of pairs of people, she comments in a footnote that one paper: 'argue[s] persuasively for the use of teams to generate protocols, where the discussions and arguments that unfold are treated as evidence for the individual reasoning of the participants. The actions of the team members can also be viewed as organized by the task of collaboration itself, although in the interest of looking at the interaction of both users with the machine, I have deliberately avoided taking that view here' (Suchman, 1987: 115)[18]. It is to her credit that this issue is so clearly and explicitly dealt with, but it remains a remarkable statement for a sociologist to make. The social interaction between the participants is downplayed, or ignored, in the interests of focusing on their individual interactions with the machine; and the talk that results from their co-presence is used to shed

---

significance of social interaction for the analysis of behaviour; however, any evaluation must remain conjectural until this position has been more fully articulated.

[18] Suchman consistently includes speech within the category of action; hence I read 'action' here as referring back to 'discussions and arguments' in the previous sentence.

light on the latter relationship. This explicit marginalisation of the social relations between the co-participants can be plausibly related to the requirements and discursive structure of the field of HCI: for it is of course 'the interaction of [..] users with the machine' which is of primary interest[19].

Let us unpack this issue a little further. On the one hand, I am arguing that talk is being treated as a means of communicating information about the user's reasoning to both the other participant, and to the researcher. But beyond this, I am pointing out that despite Suchman's inclusion of speech within the category of situated action, she only analyses it in terms of its communicative function. All the other things that talk can *do* are bracketed out. One might surmise for example, that talk in this situation might be used to cover embarrassment, to rationalise mistakes, to establish solidarity in the experimental situation and so forth. The selection and analysis of material avoids discussion of these social functions of talk.

Suchman is quite explicit about the way she will treat talk, and the motivation for it, and her analysis is a very effective one within the parameters that she sets out; I am not therefore concerned to argue against the validity of her approach. I am rather suggesting that in her analysis we can see particularly graphically the process whereby a re-presentation from a different intellectual background takes on many of the features that characterise both the discourse of HCI in general, and its core discourse of cognitive psychology in particular. However, I do think there is a methodological flaw in her general position which results from the restricted view of talk that she adopts in her empirical analysis. Part of her argument, and an important part, is that plans should be seen not as mental programmes that are drawn up prior to action but as ways of accounting for action in rational terms. The suggestion is that accounts have an overtly social function in that they work as public displays of rationality; there is an unavoidable element of ironic contrast then between what people say they think, and what they

---

[19] It is also noticeable that the talk that takes place is referred to, in psychology's terms, as a protocol.

actually think[20]. Yet in the empirical materials, talk is treated primarily as evidence for the latter. This is therefore a theoretically inconsistent formulation (although the practical effect of the inconsistency on the force of her arguments appears to be less than might be expected).

I have argued in this section that the social is downplayed in Suchman's critique of cognitive theory, in that the discursive relations between the participants are neglected in favour of the attention given to the individual's reasoning and her interaction with the machine; and I have suggested that this can be fruitfully understood in terms of the interplay of different discourses[21]. I will close this section by raising again the question of the relation of the individual to language. Treating talk as more than simply communicative suggests a sense of the social in which individuals orientate their utterances to the people present and the circumstances of the moment, however broadly that may be conceived. However, this preserves a model of social interaction in which the individual remains the source of his or her utterances. In chapter two I alluded to a different view in which individuals are seen as constituted in and through language; the subject is framed within discourse, and speaks from subject positions that are created in language. The significance of this view will be demonstrated more clearly in the empirical chapters, but it is worth noting that it poses a fundamental challenge to the notion of the individual that is central to cognitive psychology, and preserved within the latter's ethnomethodological and other broadly sociological critiques. In this sense, it is not a 'human-centred' account.

4.4. The implementation of sociology: conversation analysis

It is notable that, of all possible sociologies, it is conversation analysis that has been perceived to have the most relevance to HCI, and which dominates sociological contributions to the field.

---

[20] I say only 'an element' because, as has been noted, Suchman is elsewhere careful to include speech within the category of action.

[21] For now I leave on one side the question of whether the interaction between person and machine could also be described as social. It is discussed further in chapter five.

One of the ironies of this situation is that, as Gilbert et al point out, it has had at best a marginal status within sociology but is now in the position of being 'sought out by computer scientists who see its potential for assisting in the design of human-computer interactions' (1990: 242); in other words, its relevance can be stated in terms of its practical value for design practice. This is an unusual situation for a descriptive sociology to be in: it is being suggested that its knowledge of one form of interaction can be implemented in the construction of further, possibly inherently different, interactions, or in McIlvenny's terms, 're-embodied' (McIlvenny, 1990). It suggests a central critical question, which is the focus of the discussion that follows: what claims for the status and character of this knowledge are implied by assertions that it can be implemented in this way? A number of further questions can be derived from this about the relationship of CA to HCI, the practical and performative requirements of the latter, and the suitability of particular epistemological positions for these requirements.

First, I will briefly recap some of the features of the relationship between CA and mainstream HCI indicated earlier in the chapter, and elaborate further on some of the common ground that they share. It was noted that although the attempt to incorporate conversational dynamics into systems design was sometimes put forward as, if not critical of cognitive theory, a radically different strategy to, for example, attempting to model the user (Gilbert,1987; Frohlich, 1988); however, other work which attempts to integrate CA with cognitive modelling (Cawsey, 1990) or with computational linguistics shows that difference is by no means a necessary feature of the relationship. It was also noted that, for all that sociology is glossed as having an over-riding concern with context, the case for CA's utility can be posed in terms of the universality of the conversational dynamics that it proposes (Drew, 1990; Frohlich and Luff, 1990); in this sense, it is perfectly compatible with acontextual approaches.

A further feature of CA which has some resonance with the discourse of HCI as a whole is its formalism; by which is meant its interest in the formal properties of behaviour and interaction which are described by means of specially devised notations. In both discourses, as well as in software engineering, there is the frequent implication, if not explicit claim, that formal notations stand as some kind of guarantee of representational adequacy and methodological

rigour. The basis of this in computer science generally is a mistrust of the interpretative flexibility of natural language, for example in software specification. In a curious sense, the same can be argued in the case of CA, in that the transcript stands as a pre-interpretative representation that faithfully encodes and documents, in ever greater detail, the minutiae of interaction that elude informal descriptions[22]. For example, Atkinson and Drew's comparison of ethnographic and conversation analytic approaches to ethnomethodology suggests that an important advantage of the latter is that the data is represented separately from, and prior to the analyst's interpretation of it: interpretative flexibility cannot be avoided, but its deleterious effects can be limited by allowing the reader to assess the interpretation's plausibility (Atkinson and Drew, 1979: ch 1).

The claims that CA makes about the status of its work are, as this discussion of transcripts suggests, profoundly realist. We shall see that this realism has considerable significance both for the project of implementing its knowledge, and for discussions within CA itself about the feasibility of implementation.

4.4.1. The 'practical solutions' requirement

Norman and Thomas point out that CA and HCI share a common interest in 'practical solutions to interactional problems' (1990: 62). However, the requirement for contributing to HCI is not simply that one can plausibly describe interactional strategies and accomplishments, but that

---

[22] Strictly, there are two aspects to this view of the transcript's role: the separation of data and interpretation, and the faithfulness with which the transcript is seen to 'record' the raw detail of talk. Wynne (1986) argues convincingly against the plausibility of the latter formulation. The extent to which CA practitioners assume that a CA transcript is somehow closer to actually occurring talk that it represents can be anecdotally supported by pointing to their use of the phrase 'cleaned up transcript' to refer to, for example, the kind of representation of talk found in this thesis; given the disproportionate amount of work required to produce a full CA transcript, this is in many ways a curious formulation. It is also interesting in this respect that, within published texts, sections of transcript are often represented by the use of fonts that resemble 'primitive' typewriter copy: see for example Luff et al (1990) and this thesis.

one can make a case for their relevance to system design. Sociological studies of computer use may be full of insights into human-computer interaction as a cultural phenomenon, but unless they address themselves to the general problem of, in some way, improving system design, they will fall outside the boundaries of HCI discourse.

The effects of this requirement can be clearly read in Suchman's conclusion. Having presented a convincing, theoretically coherent, and empirically based critique of the assumptions about human action that underlie cognitive science in general, and the design of one artifact in particular, and having demonstrated that these assumptions make a crucial contribution to the problems that users have with the machine, she then proceeds, or perhaps feels compelled to proceed, to set out some alternative design strategies. These turn out to be considerably less convincing than, and in some respects theoretically unrelated to, the substance of her argument throughout: make users more aware of the machine's limitations, extend the machine's sensitivity to the user's circumstances, and (given that the last point is beyond the horizon of technological competence for the foreseeable future) employ computationally feasible alternatives such as user-modelling. On my reading , the obligation to be seen to make a *direct* contribution to design practice here results in token recommendations which in some ways spoil the force of her argument: the second recommendation being impossible, and the third being derived from the very cognitive theory that she was concerned to challenge. The irony here is that Suchman's work has profound implications for prevailing conceptions of action and interaction in HCI, and to a lesser extent, within computer science more generally: and insofar as it succeeds in modifying these conceptions, changes in relevant areas of design practice may follow. It should not have been necessary to postulate direct practical design consequences of the argument.

Frohlich and Luff (1989) argue that Suchman has successfully outlined a problem, but that its practical solution, or the beginnings of it, can be found within CA. On this account, ethnomethodology, broadly conceived, serves not only to describe interaction and its difficulties, but to improve interaction: that is, CA knowledge is implementable as an engineered solution to

the kind of interactional problems that Suchman describes. Let us now look directly at some of the claims involved in this response to the performative requirements of HCI discourse.

4.4.2. Relevance, realism and implementation

In the first instance, let us leave to one side the central question of how far conversation analysis can, or should, be seen as faithfully representing its object of study, conversation, to look at the relevance of the object itself for system design. Gilbert et al justify the turn to conversation on two levels: one can assume that 'interactional procedures and strategies which are grounded in everyday life are reasonably effective'; and, following from this, people's tacit familiarity with these procedures suggests they will have less difficulty 'communicating with computers' which incorporate some of them (Gilbert et al, 1990: 235). The critical question however is avoided: to what extent does conversation serve as a useful model for the distinct activity of using a computer? Of course, if they are both described as instances of the general category of 'interactional' or 'communicative' activity, then their mutual relevance appears self-evident; but this characterisation is by no means given. For example, both Anderson (1986) and Kammersgaard (1988) suggests that there are a number of possible 'perspectives' on the phenomenon of computer use: the computer can be viewed as a medium, as a tool or a dialogue partner, or the computer and its user can be collectively viewed as a system. Kammersgaard notes that all of these perspectives have a particular value in different circumstances, and that a flexible conception therefore has advantages: that said, he regards the dialogue partner model as the least useful in that it employs a conception of computer use which does not adequately characterise current directions in interface design, partly because it is tied to some untenable assumptions about the feasibility and desirability of engineering explicitly dialogic natural language interfaces.

One implication of this is that the relevance of conversation to interface design should be differentially assessed according to the specifics of the system in question. Gilbert et al's utilisation of CA for a speech-exchange system would seem to be less controversial than the application of the conversational model to direct manipulation interfaces in which the linguistic component is written, as in Frohlich and Luff (1990). In this respect, Frohlich and

Luff's claims are particularly strong: they argue that the dynamic and interactive qualities of computer interfaces minimise significant differences between written and spoken text, and that the expectations that people bring to the computer are shaped by their mundane experience of conversation. They go so far as to characterise the use of a system which has a direct manipulation interface as 'conversation'.

In defence of these claims, Frohlich and Luff put forward the interesting argument that the empirical study of interactions with this system would support or undermine their case by showing the extent of difference between it and the known features of conversation (ibid: 188-9). There is a problem with this however; for as Suchman notes, the application of a pre-determined methodology with its own coding scheme to human-computer interaction will prescribe the character of the phenomenon (Suchman, 1987: 114)[23]. Since CA will inevitably produce a version of conversation, it is unlikely that it would be able to demonstrate its own irrelevance. The empirical answer to the question of relevance is therefore an inadequate one. It is however of particular interest in that it demonstrates clearly the extent to which CA tends to see its knowledge as reports, and rarely, if at all, as products.

Before exploring CA's realism further, a further dimension to the question of relevance should be noted. A case can be made for the relevance of CA which does not imply that computer use is analogous to conversation, if it is claimed that CA's expertise extends to interaction in general. There is no consensus within CA on this point. Sharrock and Anderson, for example, argue for the specificity of CA to conversation (Sharrock and Anderson, 1986: 80), whilst Bilmes (1988) argues for its generality; within discussions pertaining to HCI, the latter position is taken by Norman and Thomas (1990) and Drew (1990). In both cases their arguments have a two-fold basis. The first turns, again, on the generality of the conversation model, in that expectations from mundane conversational experience are carried over to other forms of interaction[24]. The second is that the

---

[23] Suchman's comment is directed towards the coding of experimental data, and the relevance of the observation to CA is not addressed; however, she does devise a new transcription format for her study.

[24] On this point, Drew, speculating in the absence of empirical materials, is somewhat more cautious.

methodology of CA is suitable for the domain, since it can provide the 'naturalistic studies' that are required for a better understanding (Drew, 1990: 33); generality here is premised on methodological realism.

The depth of realist assumptions within CA can be seen in debates about its applicability. For example, Button argues articulately against the conflation of 'rules' as formulated within CA and the conception of rules within, for instance, computational linguistics. Such a conflation, he argues, mistakenly characterises the rules postulated by CA as internal representations which govern behaviour; whereas what is intended is a characterisation of conversation in terms of speakers' normative expectations and commitments. Drew (1990) makes a similar point. The rules described by CA are things that *'people display an orientation to'* (Button, 1990: 78, original emphasis); doubts are therefore cast on the feasibility of attempts to encode them within computer programs[25]. The point is never acknowledged, in this or similar debates, that CA rules can be described in a third way: not as determinants of behaviour, nor as entities towards which people orient, but as features of conversation analysts' accounts. On this reading, which is the one to which I subscribe, the rules and structure of conversation as described by CA, are the products of its discourse. A further example of CA's realist commitments is Gilbert et al's discussion of possible readings of Garfinkel's documentary method, in which the reflexive uncertainties that it implies, as demonstrated and developed in Woolgar's work for example, are completely ignored (Gilbert et al, 1990: 255)[26].

These realist formulations and claims are of crucial significance for the general attempt to implement sociological knowledge: for CA's claim that it has something to offer to the enterprise of designing and engineering systems seems to rest, in general, on a characterisation of

---

[25] Button's position is heavily qualified however, and is aimed more at claims that are made for such attempts (in terms of the replication of conversation) than at their practical feasibility and desirability.

[26] Woolgar's development of the documentary method is discussed, and used as the basis for an analytic approach, in chapter 7.

its own knowledge as a set of 'findings'[27]. The interconnection of both claims can be seen, in the opposite direction, in Frohlich and Luff's somewhat bizarre assertion that the fact that they were able to use CA findings 'relatively directly' as a basis for both deciding between different design options, and of implementing those decisions attests to the quality of CA accounts (Frohlich and Luff, 1990: 189). If it's engineerable, apparently, its probably true[28].

My contention is that claims that CA knowledge is implementable, and realist claims that such knowledge is a set of 'findings' about the phenomena being studied, are mutually supportive; and further, that if this is so, then the predominance of CA within HCI can be related to the strict performative requirements of the latter, and the realism of the former, and not simply and intuitively to their common interest in linguistic and interactional phenomena.

4.4.3. Principles and prescriptions

Finally, I will briefly note two further aspects CA discourse and practice in this field: these concern the advice that CA has to offer system designers, and have some bearing on the relationship between CA and HCI.

Firstly, there is frequent emphasis on the 'principled' character of CA's findings, or solutions, in justifications that are made for its employment in HCI: for example in Frohlich and Luff (1990: 189), and more emphatically, in Norman and Thomas (1990: 53). One is reminded here of Long's dismissal of work which, however effective, is not explicitly principled (Long, 1989: 20): this was discussed in the last chapter. Norman and Thomas give an empirical example of interactional difficulties, and suggest advice that could be given to the designer, or more

---

[27] In cases where the claim is based on methodology, the realist assumption is, as I have argued, equally fundamental.

[28] Mulkay (1979) argues convincingly, on empirical and logical grounds, against the widespread assumption that the successful practical application of knowledge can be viewed as its validation; he also notes, and this appears to be particularly relevant to the claims being discussed here, that 'success' is itself subject to considerable interpretative flexibility.

ambitiously, design guidelines that could be drawn up, which would draw on CA work. Noting that in some senses, the advice they would give is equivalent to the requirement that the system should provide 'feedback', they suggest the advantages of a principled solution to the problem in terms of a more detailed understanding of the many relevant facets of interactional process which CA reveals, and to which the designer could be 'sensitise[d]' (Norman and Thomas, 1990: 61): the problem for this reader however, is that it is by no means clear how the 'principled' design recommendation would differ from the more intuitive and pragmatic one[29]. I do not wish to imply that if it cannot deliver direct and distinctive practical solutions, then CA has nothing to offer; clearly, that is not the position for which I am arguing. It is however Norman and Thomas's implicit position (ibid: 62). On their own terms, arguments for the necessity of principles are therefore not convincing. They are however characteristic of HCI discourse as a whole.

The second point concerns the issue of how to move from a description of phenomena to a prescription for their design. This is discussed further in chapter six, where I suggest that it is a problem which is both recognized within, and of fundamental importance for, the discipline of HCI. It is of course by no means the case that sociology as a whole has no concern with matters of application, but it is true, on the whole, that ethnomethodology, conversation analysis and descriptive sociologies in general have been disinterested in, if not disapproving of, attempts to make any kind of prescription for social phenomena[30]. The CA position in HCI is therefore particularly interesting, and the shift of domain is significant on two different levels. On one

---

[29] There may be a further problem here, in that the concept of 'feedback' has its own theoretical and 'principled' history within psychology.

[30] Agre suggests that both anthropology and (the engineering discipline of) AI share a common interest in moving beyond descriptions of observed forms human activity towards explanations of 'what else it could be instead' (Agre, 1990: 377). I believe this is a misreading of anthropology, and of the kind of sociology discussed here: there is a concern to show the cultural and contingent character of social phenomena, and to dissolve their solid and inevitable appearance; suggesting concrete alternatives, or trying to improve or redesign phenomena is a different matter however.

level, it forms the basis for prescriptions: what is a description of mundane speech becomes a prescription for artifact design. On the other, it prescribes in the technological domain in a manner which it largely eschews within the (conventionally defined) social domain. The relationship between the disciplines of CA and HCI is not therefore to be understood simply in terms of their similarities, but in terms of the dynamic interaction of two discourses, and of the changes that takes place as one accommodates to the other.

4.5. Conclusion: discourses, theories, applications

The purpose of this chapter has been to review sociology's contribution to HCI, and through the analysis of similarities and differences, to explore its relationship to what I have loosely called the core discourse of HCI. I argued in the first place that the frequent perception, within HCI, that sociological accounts provide a 'bottom-up' methodology which attends to the empirical details of social context misconceives the heterogeneity of 'social' perspectives, and neglects both the interpretative flexibility to which 'context' is subject in any account, and the extent to which it is a discursive construct: in this last instance, HCI replicates empiricist assumptions that are frequently found within ethnomethodology itself[31]. I then examined a sociological account which is critical of cognitive scientific theory and suggested that it shared significant similarities with the work it challenged: notably, that discourse was treated as evidence for users' reasoning processes, which were the focal point of interest. Finally, I looked at claims that sociology could make a practical, and in some cases implementable, contribution to HCI discourse and artifact design, and suggested that such claims could be related both to the requirements of the wider discipline, and to the realism of the form of sociology that has the predominant role here, CA.

---

[31] However, it may be that the empiricist reading of context within HCI can be related to the core discourse of psychology, in that the predominance of the experimental paradigm gives studies of people in everyday environments a significance that they do not have within sociology: for example, some of this work is referred to as 'ecological' or 'realist' (Forrester and Reason, 1990: 292).

All these arguments bear on the central issue, introduced in the previous chapter, of the relations between discourses and the problems of interdisciplinarity. I suggested there that HCI, for all its multidisciplinarity, can be defined in terms of parameters which are largely derived from a core discourse, and which limit the scope of possible contributions from other discourses. This chapter has continued the argument, in that the sociological work that has been examined seems to fall firmly within these parameters, even as it proclaims its difference. Nevertheless, some qualification is necessary. It is evident that this process cannot be described causally as a simple unilinear process. For the dynamics of the relationship have been described in a number of ways: as already existing similarities, but also as changes within the methodological and theoretical frameworks in question as sociology moves into a new discursive field; whilst these changes can be further differentiated into, for example, perceptions of sociology within HCI, and claims made by sociology for its own work. The complexities of this process need further study to be understood in detail; but an adequate description in general terms is that the parameters of HCI discourse create the conditions of possibility for sociological contributions. Thus, the predominance of an overtly realist strand of sociology, CA, can be understood with reference to its conformance with these parameters, whilst changes of emphasis and substance that take place in the new field illustrate Fish's point that even where there were prior differences 'the imported product will always have the form of its appropriation rather than the form it exhibits "at home"'. It is 'brought into a practice .. in terms the practice recognizes' (Fish, cited in Messer-Davidow and Shumway, 1990:262-263).

For sociology to *be* HCI therefore, it should conform with some basic requirements; whilst significant differences that remain will tend to lead either to the work being ignored or to its re-interpretation in more familiar terms. The following chapter, which begins the presentation of empirical materials, can be read as an illustration of a different approach to studying human-computer interactions which would not conform to these basic requirements: notably, it would be difficult to draw any *direct* design conclusions from it. However, I contend that, insofar as it attempts to contribute to the understanding of such interactions as cultural phenomena, and addresses itself to prevailing conceptions of interactional phenomena within, and outside, the

discipline, it suggests that a more sceptical form of sociology has something of value to say about human-computer interaction.

Chapter 5

Describing interaction with computers

So far I have attempted to elucidate a theoretical approach to discursive phenomena, and to examine some of the consequences that such an approach has both for the study of the discipline of HCI, and for attempts to contribute to it. Broadly stated, the view that language should be seen as constitutive rather than reflective, and the closely associated position that disciplines each have their own distinctive discourses which form domains of objects, imply difficulties for the project of inter-disciplinary collaboration as it is ordinarily conceived. I have argued that many of the sociological contributions to the field to date can be seen to be shaped in significant ways by some of the prevailing features of HCI discourse, and furthermore, that the theoretical position that has informed this analysis is one to which neither HCI in general, nor its sociological contributors would wholly subscribe. In this chapter, I want to begin to explore some of the implications of this position for the empirical analysis of computer use.

Since a major part of my argument is that HCI discourse as it presently stands has a formative and constraining effect on potential contributions, the analysis that is presented in this chapter does not respect the boundaries of what may be considered valid and useful knowledge for HCI. It is rather located within an explicitly sociological discourse, and it makes some connections with debates taking place within sociology: in particular, the debate on agency, technological artifacts, and the scope of sociological discourse that has been recently raised within the sociology of scientific knowledge (Latour 1988a; Collins and Yearley 1990). This is not to argue

that it contains nothing of interest for HCI: for example, the theoretical application of a relatively sceptical view of language, considered as a mode of representation, has practical implications for some approaches towards system design, whilst the debate on agency has some relevance for HCI, and in some respects parallels discussions that have taken place there. However, practical applicability and relevance have not been the chief criteria for what should be argued and included, whilst the reflexive implications of the theoretical position taken here suggests that the status of the account itself must be seen as undecidable, and therefore radically different from a presentation of 'findings': the more usual requirement for a contribution towards the design of better computer systems.

5.1. Materials

The empirical materials for this discussion comprise transcribed videotapes of small number of 'interactions' selected from a wider collection. One of the chief criteria for the selection has already been discussed in some detail in chapter two, that is, a preference for situations in which people are talking, given some of the difficulties posed by silence for sociological analysis[1]. This is not the only criterion however: other material in which people speak whilst using the computer has been discarded. For example, material gathered from two days of observation and filming at an Open University summer school course on mathematics has not been used, partly because of indifferent technical quality, but more fundamentally because the computer was relatively transparent in users' discourse in comparison with the specific mathematical problems which were their main concern. On other occasions, attempts by users to explain their use of the computer for my benefit yielded talk in which my presumed research interests appeared to be by far the most significant factor. All of this material is in principle perfectly usable as sociological data: in the former case, the transparency of the computer in users' talk is a

---

[1] To some extent this requirement is implicit in the basic theoretical orientation of the thesis as a whole: however, the tentative suggestion made in this chapter that software could be considered as a form of discourse does raise the possibility of some kind of textual analysis of silent interactions. I have not pursued it however.

suggestive topic for further investigation, whilst in the latter, the significance of the researcher's presence by no means invalidates the material unless one is committed to some version of methodological 'naturalism' (Hammersley and Atkinson, 1983). Selecting from non-silent interactions was done primarily on the grounds of the intrinsic interest of what was said.

Details of the transcripts referred to, and the simple notation that I have adopted can be found in the appendix. Two of the sessions, 2.1 and 2.2, involve interactions in which talk takes place between two people who are seated at the same computer. In 2.1, a user (U) is being shown by a colleague (C) how to produce a simple document on Microsoft Word on a Macintosh. 2.2 is part of a consultation that took place as part of a project which was designed to assess the possible merits of adaptive interfaces via experiments which measured the performance of different users, and more importantly, user types, on different types of interface. Here, the consultant (Y) looks at the software in the company of the programmer and designer of the experiments (F) with a view to assessing its adequacy for the experiment and recommending any last minute changes. (This data is examined in more detail in chapter 6). Finally, in 1.1, talk takes place between the user and myself. The occasion is an evaluation of a prototype of a piece of software that had been produced at the Open University, the 'Object Editor': it was given to a number of people to test out in the computing department, and in particular to anyone who had an interest in HCI. I had already looked at it and suggested to a colleague who also works in HCI that it might be interesting to film his first use and evaluation of it. At the time that this took place, I did not have a clear sense of the central importance that discourse would have for my thesis: the conversation that took place was therefore relatively undirected by me, and its subsequent examination was instrumental in my change of direction from a version of 'studies of work' (Lynch et al, 1983) to an emphasis on discourse.

5.2. Discursive constructions of software, user and their interaction

The theoretical orientation towards discourse that informs this thesis problematizes the study of human computer interaction in at least two respects. On the one hand, 'interaction' itself has

to be seen as a discursive construct, as a characterization with certain theoretical commitments, and not as an empirical given. On the other, the talk that takes place at and around the computer cannot be treated as a report or commentary upon either the details of machine input and output, or the user's perceptions of them. However, it is of interest in its own right, and moreover, it can be fruitfully related to the specifics of computer use in different ways. In this section I begin by looking at the relation between computer and users as constructed in the latter's discourse; that is, I examine users' characterizations of the interaction, and specifically at the attribution and construction of agency. I also consider the possibilities of treating programs themselves as a form of discourse. I wish to emphasise here that, in line with the treatment of language throughout the thesis, I am not making any claims about speakers' attitudes to, or beliefs about computers: 'attribution' is intended to denote a linguistic, not a cognitive phenomenon.

In one sense the focus on users' attributions could be seen as the standard ethnomethodological move from resource to topic (Zimmerman and Pollner, 1971). The intention here however is not to remove or resolve problems of interpretative flexibility by attempting to minimize the constructive work of the writer, since I share Potter's scepticism about such claims (Potter, 1988). Instead, I wish to look at the process whereby both computer system, or elements of it, and user are constructed as agents or subjects in the talk that goes on around the machine. The significance of these (users') representations is not that they have any epistemological priority over any other, but that they are themselves part of the phenomenon being studied, and more pragmatically, that they can be *used* to call into question other characterizations of computer and user. Their limitations will be addressed in due course.

This is by no means the first piece of sociological work that has addressed, or made reference to, the question of the attribution of agency to computers[2]. The focus of much of this work is the question of machine intelligence, and the socially sanctioned attitudes towards it which can be read in talk about machines. The point of interest here differs in that I take questions of

---

[2] See for example, Turkle (1984), Woolgar (1985; 1989; 1990).

intelligence, consciousness and intentionality to be distinct from the question of agency, and have no direct interest in the former[3]. I am primarily interested in the ways in which a degree of autonomy is granted to artifacts in discourse. I should stress that I take it as given, from observing the regularity of its occurrence in computer use, that examples of the ascription of agency to computers can be easily found. The interest of the attributional work that is described here lies in its detail, in some of the distinctions that are made in describing both agency and the implied relation between agents, not in the fact of its existence.

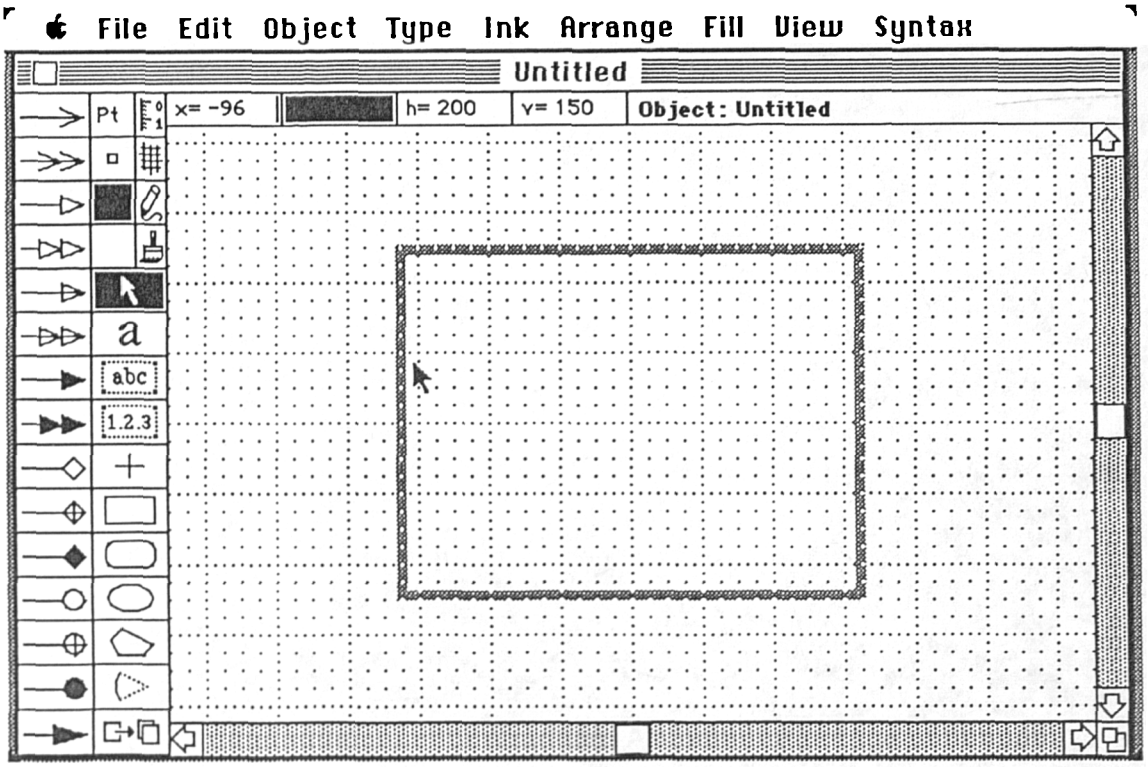
5.2.1. Attributing agency to different parts of the software

Whereas discussions of 'attitudes' towards computers, notably Turkle (1984), tend to treat the computer as a unitary entity, examination of some of the features of discourse in the presence of computers reveals that, in many instances, distinctions are made between different parts of the software[4]. This is particularly evident in the evaluation of a prototype graphics package, in which the presentational style suggests a diverse collection of entities with different functions (see figure).

---

[3] By this I mean not that such issues are incompatible, but that arguments for the plausibility of granting agency to technological artifacts can be effectively made without making any claims for their intelligence; Latour (1988a) does precisely this.

[4] No irony is intended here, attitudes and discourse being two distinct analytic objects (Potter and Wetherell, 1987).



Figure

The user describes certain parts of this interface, for instance particular icons, in terms of their availability for acting upon other parts. For instance, when attempting to remedy what he takes to be the currently defective state of the software, he explains his actions in the following way:

(1.1: 2)

J: I thought if you click .. (clicking everywhere to escape loop) you can't even click onto something else in order to get out of it,

G: Yeah

J: that's the daft thing about it.(clicking everywhere to escape loop) You think that by clicking to get out of it,

G: mmm

J: you know, you're clicking onto something else,

G: mmm

J: the squares or the .. alphabet or something.

Here, a number of distinctions are made. The repeated phrase 'get out of it' formulates the defective state of the program to be remedied, which is distinguished from those visible elements of the software which are expected to provide the remedy if correctly activated: clicking 'to get out of it'. At the same time, the defective state, which is deduced from the user's inability to control the behaviour of the cursor in the centre of the screen, is itself associated with one of these icons: hence the attempt to 'click onto something else'. In this instance, a limited form of agency is attributed to those elements that could be activated; they can be used to control the behaviour of a conceptually distinct part of the software. In terms of the debate about the plausibility of granting agency to technological artifacts, this conceptual distinction is of ' some interest since agency is granted not to the computer, nor to the software it is running, but to certain symbolic components of the software. The precise degree of agency that is granted is however subject to some flexibility. For example, in the informal report passed back to the software designer, this area of difficulty was described as follows:

```
(1.1, Document 1: 1).
I clicked on all the icons on the left side but none of them got me
out of it.
```

This is a slightly stronger formulation; commenting that despite clicking on these icons 'none of them got me out of it' grants more agency than the previous 'you can't even click onto something else in order to get out of it'. The difference between the two statements is a simple grammatical one between the user and the icons as subject of their respective clauses; it can be read as the difference between instructing and then relying upon assistants to remedy the fault, and fixing it yourself with the use of tools. In the next extract, the user attempts to recover the situation by using some of the pull-down menus at the top of the screen, only to discover that these cannot be activated in the present state. Both grammatical forms are used consecutively:

(1.1: 3)

J: That's dreadful .. that is absolutely horrendous that .. It doesn't even access the menu, you can't access the menus from there or anything

Here both user and 'it' are both present as subject. 'It' is unspecified but, to judge from the user's actions, it might refer to the defective state of the software, to the cursor, or to the software as a whole, but not to the icons that were the subject of the previous extracts. Moreover, the user's discourse builds more complexity into the implied functions of, and relations between, the different visual elements of the system: in addition to the defective state, which is causally associated with certain icons, and other icons which could reasonably be expected to be of some use in remedying that state, it is now asserted that the defective state prevents the activation of yet another part of the interface, the menus. What is described is a dynamic network of different interacting elements, to which variable degrees of autonomy can be attributed, and in relation to which the user's own ascribed role as agent is also subject to variation. It may well be that the indeterminacy of the user's role can be related to the perceived complexity and heterogeneity of the software.

5.2.2. The identification of user and software

The question of the discursive relation of user to software, or to elements of it, can also be approached through an examination of the way in which he or she is identified with certain aspects of it. An as yet unremarked feature of the first two extracts is that, regardless of who or what is to remedy the defective state of the program, the phrases 'get out of it' and 'got me out of it' posit an interesting relation: it may be the program that is defective, but it is the user that is somehow within, and constrained by, the resultant state. This kind of identification of user with some feature of the software seems to be a common formulation across different occasions of computer use. Here are two further examples from a different interaction:

(2.2: 14)

Y: Again this is assuming that by typing help that they've gone into the catalogue ..

F: Well with this you're in it anyway yes .. Yeah I mean I haven't done anything like you have to go into a certain area and that's the catalogue, there's nothing sort of like logging in or anything, I mean you're just in there anyway

(2.2: 24)

Y: If I'd pressed that one, would it have done exactly the same {or would it just} have got me back to the previous screen?

F: You would have gone back to the {}

Y: Um .. for the other ones, when you return, did you come back to that same state?

This is a pertinent place to stress the limits of what is being claimed here. Were these examples of discourse around the computer being offered as evidence for the user's interpretations of, views on, or even cognitive relation to the software, they might suggest that the user thinks of him or herself as actually going 'into the catalogue' or being taken 'back to the previous screen' and so forth. This is not the intention. In asserting a form of identification of user and part of the software, I am suggesting only a figurative feature of discourse. Wittgenstein's discussion of language games is especially relevant here. Firstly, in a general sense, it suggests an approach towards the study of language use around the computer which treats it as a distinctive form of life and analytic object in its own right, not as evidence of something else. Secondly, games, which serve as a model for the discussion of different forms of language (Wittgenstein, 1958: passim), may provide a useful analogy for the phenomenon described here. The following utterance, which is a piece of advice to a user from a colleague who is instructing her in the use of an unfamiliar software package, provides a clue:

(2.1: 8)

C: Err now, go to, take the arrow to format, at the top

The initial 'go to' is followed by the more precise instruction, 'take the arrow to'. The analogy I have in mind is that of a board game, in which a counter serves as a representation or representative of the player. Here, the arrow serves a similar function. The figurative identification of the user with some aspect of the software can be related to some visual element which is manipulated by the user, such as cursor or pointing symbol, and which acts as their representative. The presence of this element in some spatial or conceptual area of the software can stand for the presence of the user. This can be more or less explicit. The presence of the user within a defective state formulated in the first three extracts above (from 1.1) can be clearly related to his inability to satisfactorily manipulate the arrow, whilst the last extract (from 2.1) makes the connection for us. The two extracts from 2.2 are less obviously consistent with this view. In the first of these, a non-graphical, command based interface to a mail order catalogue is being discussed. The problem implied by the first speaker is that it is not made clear which conceptually distinct area of the software can be accessed at that point: hence the statement that it is assumed that 'they've gone into the catalogue', and the response 'I mean you're just in there anyway'. In the second, it is the screen that is presented to the user which is associated with the user's presence within a given state. In both cases however, it can be plausibly argued that it is still that element of the software which is directly manipulable, what might be called the on-screen input device, which stands for the user. To be 'in the catalogue' is to able to enter the commands that are appropriate for that mode; whilst '{would it just} have got me back to the previous screen?' asks which screen will be available for further input. (It is worth commenting in passing that one element of the software - 'If I'd pressed that one' - is again ascribed limited agency in assisting the user in this). The discursive identification of the user with some part of the software or its behaviour therefore appears to be directly related to that part which he or she can access and manipulate by means of an on-screen input device/representative.

5.2.3. The constraints of a technological society

The relation between user and elements of the system that is constructed in discourse around the computer can be analysed in terms of a topic that is implicit in much of the data already quoted, and which is a familiar aspect of much computer talk: the way in which the system is differentially described as either malleable and responsive, or constraining and relatively autonomous with respect to the actions of the user. Both of these characteristics are available as explanatory resources for the user, most notably on occasions when the software appears to behave unpredictably.

Whilst I have, in the discussion so far, attempted to stress that a software package can be, and is differentiated in terms of some of its constituent elements, the significance of the fact that computer behaviour derives from the simultaneous presence and activities of many different programs has been neglected. Thus, for example, the apparent software fault encountered by the user in 1.1 is described in terms of the interaction between the software package and the operating system:

```
(1.1: 4)
J:    It doesn't even, you can't move it .. you can't scroll across
      the screen up or down .. it disables the whole system apparently
```

This is a interesting instance of the documentary method of interpretation at work: the user's inability to manipulate the software is not only confidently ascribed to a defect in the system, but the manifestation of that defect is ascribed to its corruption of another collection of programs which controls many of the computer's basic functions. Again, the heterogeneous character of the described artifact is evident. One important consequence of this heterogeneity is that similar formulations can be found in the discourse of a wide class of users. Even where, as in 2.2, one of the participants wrote the software that is being run and evaluated, problems with its behaviour can be described in terms of the parts of the system as a whole which are not under her direct control:

(2.2: 20)

(System delay)

Y: What have I done?

F: I don't think it's decided to garbage collect

Y: Oh no. Oh it's come back up

F: No it's back

Y: It's just got that slow

F: Sometimes it does this

The first utterance by the person who is testing out and evaluating the system assumes that the software is responding to something that she has initiated. The software had been specifically designed for an experiment involving a number of explicitly defined tasks. Consequently there had been some problems when Y attempted to activate parts of the software that had not been implemented; hence her assumption that the delay was the consequence of her action. The designers response addresses, and tentatively discounts, the possibility that part of the system not under her control has 'decided to garbage collect'. Garbage collection is a function activated by KEE, the software package which was used to write the program being tested, and within which it runs; considerable autonomy is here granted to this process. The final utterance suggests that this is an (unexplained) feature of the software's behaviour that has been observed on previous occasions. The point of interest in both F's utterances is that even though she programmed the system whose behaviour they are discussing, she describes it as a relatively autonomous entity. It may well be that the problem could be directly traced to a line of her own program code, but since the programs she writes are part of, are constrained by, and interact with other programs over which she has less or no control, the behaviour of the system is not described as the mere instantiation of her instructions. The articulation of the constraints that the software puts upon the user's attempts to manipulate it are therefore not confined to the discourse of what computer science calls the 'end-user'. Here is an example of the programmer explaining a behavioural trait of her program as the result of another program's failure to adequately respond to an instruction:

(2.2: 1)

Y: Is this just to clear the screen?

F: Yes, I've just got to set .. This is really odd, the, the screen that comes up at the end to tell you, umm, what you've chosen, it disappears straight away

Y: That's, that was something that was happening before?

F: Yes, like that, but if

Y: {} see that

F: Yeah, no the thing is I set it up .. This line of code is actually written in there but it doesn't seem to respond to it, but when I put it in there it's ok

This suggests a relation towards program authorship which is analogous to Heidegger's conceptualization of the relation of a speaker towards his or her language: in the same way that a statement is fashioned from, and produced against the background of a language which is 'always already there', the set of instructions that comprises a computer program not only take its place within, but also derives many of its effects from, a collection of other programs which are already in place, written by other people, and in most cases less familiar and less predictable. The metaphor of a 'society' of programs is in many ways a tempting one in that it suggests both the significant role of what already exists in shaping the form of a program, and the difficulty of fully predicting and controlling the effects that it will have as it interacts with other programs. The point of such a metaphor, which could doubtless be refuted with reference to any number of allegedly definitive social attributes not possessed by the computer (Woolgar, 1985), is simply that it again stresses that discussions of how far agency is, or should be, attributed to technological artifacts such as computers neglect the significance of the latter's perceived internal complexity. Within users' discourse, distinctions are not simply made between artifact and human, but between elements that comprise the artifact, elements which are differentially controllable.

5.2.4. Morality and authority

The metaphor of language/society can be explored a little further. I have suggested that software is shaped by what already exists - for instance, the syntactic rules of the programming language in which it is written, the requirements of the particular compiler which will translate it into another form and so forth - and that the task of specifying its likely behaviour is complicated by its interaction with other programs. These may be seen as part of the reason why software behaviour is described as relatively autonomous by users, even by some of those who write software. What I am calling social here are those aspects of the internal relations of the computer system, as represented in discourse, which have an impact on the behaviour of a given program. An understanding of its denotative and connotative effects however requires a wider sense of the social, encompassing relations that are external to the computer system, in order to understand the meanings that it acquires in use[5]. These external relations need not be conceived as exclusively human (Callon, 1987; Latour, 1987). Just as a statement or a text derives its meaning from its relation to, and place amongst other statements and texts, so the program's relation to other programs is significant. Of course, the objection can be made that this elides the distinction between programs and human uses, perceptions and descriptions of programs; but for the moment I wish to leave such objections to one side and utilize a model that includes both humans and artifacts. Insofar as other programs are seen to establish legitimate conventions for software behaviour, then programs can be described in moral terms, as obeying or violating supposed norms of acceptable behaviour.[6] The evaluation of the prototype graphics package (1.1) which is new to the user, and which contains a number of apparent defects at this stage of early implementation, is, viewed in this light, equivalent to a breach experiment (Garfinkel, 1967) in that it makes visible some of the relevant conventions.

---

[5] This is analogous to a common strand in continental theories of discourse: that is, that speakers and writers 'only have limited control over the connotative effects of [their] utterances' (Burton and Carlen,1979:31).

[6] 'For members not only are matters so about familiar scenes, but they are so because it is morally right or wrong that they are so' (Garfinkel, 1967: 35).

I do not wish to claim any kind of universality for the conventions and moral attributions that follow. For instance, the fact that both individuals work within the field of HCI, a field which aspires to formulate principles for correct software design, is probably highly relevant to the form of the attributions made[7]. Rather, it is the invocation of normative expectations in discourse, in whatever form, and the availability of other software as a discursive and explanatory resource, that are of interest.

The user's discourse in a number of places explicitly relates the package to the perceived conventions established by other Macintosh software with which he is familiar, and which it appears to resemble:

(1.1: 9)

J: Is that the lassoo? .. Oh I'm back into that again, that thing you know {} I'll tell you what I was looking for, actually, I was looking for the lassoo .. like they have on Paint, and you just chop bits out

(1.1: 2)

J: I mean some of these icons, I don't know whether it's partly copy .. you know, because of copyright or patent or whatever, but certainly (points to icons) there's no conventional approach to using the Mac interface with a standardized .. icon

In the first extract, the user accounts for his actions as attempting, unsuccessfully, to locate a function that is present in a Macintosh software package. The second extract goes further and attempts to account for the lack of resemblance of some elements of the software to perceived design conventions. The validity of the explanation need not concern us: it is the fact that an

---

[7] Chapter six addresses more specifically the significance of the discourse of HCI for the form that particular utterances may take.

explanation of any sort is considered necessary that is of interest, since it establishes the normative expectation of similarity between this and other software packages that are deemed to be of the same general type. Where resemblance is not apparent, it must be accounted for. From this position it is a short step to criticizing design features for failing to comply with conventions:

(1.1: 4)

J: Aah .. home view .. I mean, what's that supposed to mean? .. bad menu .. doesn't obey any convention or anything that does it? I've never come across that term before {have you}?

(1.1: 9)

J: That's interesting, you can cut it out using keyboard configurations you know, control and z, mm, x then you can paste it in again using that (points to keyboard) but I think that's pretty bad as far as the Mac's concerned because there's no undo cut

Here conventions are explicitly invoked as the grounds for criticism of the design and behaviour of the software. Elsewhere, they can be plausibly inferred as the basis for strongly normative judgements on the software's apparently defective behaviour. Where the software fails to respond in the way that the user apparently expects, his expectations can be related to the background of conventions that have been described, and the family resemblance of the package to others. In the next extract, the failure to comply with the user's intentions is the basis for criticism, but his formulation of what should have happened is based upon the way he achieves such intentions in similar packages:

(1.1: 6)

J: I don't want that filled in .. why is that, why is it doing that? .. {just} because I selected a coloured box .. oh that's

not so bad .. but I didn't want that (points to screen) to replicate that colour (draws new box, clicks, shape is filled in) It shouldn't do that! The box replicated the colour of the previous one, it shouldn't have done that

The strength of this moral condemnation can be correlated with the particularly strong attribution of agency to the software. Whereas the previous extracts contain criticisms of design, and by implication, of the design decisions that underlie them, here the behaviour of the software, or part of it, is described in relatively autonomous terms. How far this correlation could be generalized to other occasions of use is an open question.

Condemnations such as this are not restricted to situations in which the system appears to be insufficiently responsive to the actions of the user, or in which it is perceived as constraining. For example, in the next extract the software is criticized for failing to constrain the user's actions:

(1.1: 3)

G: Yeah, I suspected that was wrong, cause you shouldn't have the cross when you get over onto the icons .. it shouldn't let you do that

J: No

That the software can be condemned for this underlines that it is not its inability to respond to the user's actions, or carry out his or her intentions which is at stake, but rather its violation of the framework of conventions within which such actions are devised and initiated. Such conventions are largely inferred from the behaviour of perceptibly similar software packages.

The analysis of the external relations within and from which software derives much of its denotative and connotative effects has so far stressed the relations between different pieces of software. However, it could be argued that programs, like statements, derive their meanings from the conventionally defined social context of their use. Here the stress falls on the ways in

which meaning is differentially ascribed within particular social settings, where such settings are conceptualized as human. Such an approach is, I contend, not incompatible with one which points to the significance of relations between different programs[8]. If programs can be treated as analogous to texts or utterances, then the attempt to restrict the sense of the social to interactions between humans can be treated as analogous to a particularly restrictive and currently discredited theory of signification which posits the human subject as the source of meaning. I discuss this further at the end of the chapter. There is no reason however why such conventional social interaction cannot be considered as simply another part of the network of relevant external relations within which a program acquires meanings (Latour, 1987). The remainder of this section begins to broaden the frame of analysis accordingly.

The last extract quoted suggests that a behavioural convention has been violated; the convention in question amounts to a requirement that the software should not permit a certain class of (on-screen) action. What is called into question is the software's authority, but this authority, or lack of it, is ascribed to the software by implicit reference to established features of other software. In another interaction however, the frequent attribution of authority to software serves a more evidently rhetorical and coercive function in respect of the social relations between speakers. The authority of the software is constructed through frequent assertions, by speaker C who is instructing speaker U, that there is only one permissible way of achieving certain effects: the user has no choice about the input she can make in relation to these effects. For instance:

(2.1: 3)

U: Shall I leave that on then?

C: Yes .. and um, where the cursor is flashing, that's where you have to, um, indicate what the name of the style's going to be

---

[8] However, I discuss below some of the pitfalls of assuming that the social context is simply empirically available for inspection and analysis.

(2.1: 4)

C: Ok, now we want to define the characteristics {of that} so once again we've got to go back to format character which, if you remember, is command d

Careful examination of this interaction suggests that in this instance the assertion of the software's authority is best understood as an assertion of the instructor's authority. By implying that there is only one way in which the software can be used, he ensures that the user complies with his clear sense of what constitutes 'correct' practice: the significance of this sense for the interaction and its interpretation is discussed in more detail in the next section. Here the attribution of qualities to the software can therefore be related to some formulation of the immediate social context of its use, and specifically to the interaction between the two people seated at the computer. It is worth emphasizing the point made earlier that the acknowledgement of the significance of the interaction between people, bearing in mind that some form of co-presence is a requirement for generating any talk around a computer, is one feature of the approach taken here which distinguishes it from the designation of such talk as a protocol which can be used to reveal the user's sense of what is going on.

In arguing that the relation of a program to other programs is a significant factor in the meanings it comes to have for users, in other words that it can be analysed like an utterance within a wider discourse, I am not therefore attempting to minimize the significance of the social setting in which it is used. Both aspects are available as explanatory resources for the researcher; their relative importance will vary according to the material being studied, not according to *a priori* methodological commitments.

Let us conclude this section with an extract that demonstrates an awareness of both these aspects on the part of those attempting to design an error message. The discussion tries to control the denotative and connotative effects of an error message by both trying to anticipate the aptness of the message for the likely mistakes that will generate it, but also by relating such effects to the known properties of other interfaces of the same type with which it can be identified. In

addition, part of the interest of this extract lies in the attempt to convey a suitable 'tone' through a medium which, though it may suggest a model of speech through its interactive qualities, is written[9]. If nothing else it suggests the validity of Latour's assertion that part of 'what defines our social relations' is delegated to machines, and prescribed back to people by them (Latour, 1988a: 310). Moreover, there is an implicit awareness of this process and of the inherent uncertainties and difficulties involved in attempting to control it on the part of the authors/designers. The exchange occurs after Y, who is trying out the system with a view to recommending possible changes before it is used in a series of experiments, misspells a command and is given the message 'choice must be one of' followed by a list of legitimate entries.

(2.2: 19)

Y: 'Check spelling'

F: (laughs) Yes

Y: Cause presumably that's what the mistake's going to be

F: 'Can't you spell idiot?' (both laugh)

Y: In a more {} tone

F: (laughs) Yeah

Y: I mean clearly with this type of interface the, the tone of the message is very important

F: Yes

Y: I mean that's the whole thing about these types of question and answer

F: Yes

F: so 'choice must be one of' is quite, sort of, authoritarian

F: Yes

---

[9] I am referring here only to a specific kind of user interface, and not to computers in general. The peculiarities of this kind of interactive writing have gone largely unremarked in the HCI literature (but see Poster 1990).

Y: I mean, rather than 'check your spelling' (adopting more friendly tone of voice - all laugh)

F: Oh I don't know

Y: But then that could be quite patronizing

F: Yes (laughing)

Y: Or something like 'try again'

F: Yes

Y: might be more {type of sympathetic}

5.3. The discursive construction of context

I have argued that the meanings that programs come to have can be related to both the social setting within which they are used, and their relations with other other programs. In this section I want to examine the question of social context in more detail. One reason for doing this is to counter the over-simplistic view of 'context' which prevails in HCI: a number of broadly anti-cognitivist approaches have raised the question of context in different ways, but they have been perceived as largely saying the same thing, and more seriously, some of the implications of these critiques have been ignored. Some of the distinctions that can be made between these approaches were discussed in the last chapter. Here I want to offer a discussion of context which, whilst affirming its importance, acknowledges the uncertainties involved in its representation[10]. In this way I hope to demonstrate that whilst the concept has analytic utility, it resists attempts to incorporate it within other HCI methodologies to which it constitutes a significant challenge.

---

[10] This is a necessary consequence of the view that language does not reflect but constitutes objects. Such a view also throws into question the idea that sociology's role in HCI is simply to contextualize cognitive theories which have neglected this dimension (see chapter 4).

5.3.1. Representation of context

It is one thing to affirm the importance of relating actions to context. It is another to take that context as empirically given. Garfinkel counters this quite explicitly by pointing to its reflexive character: ' ... not only does no concept of context-in-general exist, but every use of 'context' without exception is itself essentially indexical' (Garfinkel 1967:10). The representation of context can be seen as problematic on two different levels: that of the user and that of the researcher.

In social interaction, people orient towards a sense of context, and utilize it as an interpretative resource, but it is important to recognize that this sense is never given but is rather their own interactional achievement; it is 'a resource which is constructed and drawn upon in the course of their practical interpretational work' (Woolgar 1981:12). The primary means for creating and sustaining a sense of context is the use of language. This has two consequences. Firstly, it effectively broadens the dimensions of context beyond the limits of the immediate empirical situation, since participants' use of language brings into play a whole range of cultural frameworks, language games and ways of speaking. Secondly, if context is an interactional achievement, there is no guarantee that a consensus will be reached. A way of reading the interaction that is described in the next section is that context is contested rather than agreed.

A more fundamental uncertainty derives from the researcher's attempt to formulate the context[11]. Given that there will always be innumerable candidates for what should be taken as relevant features of any setting, and given the indeterminacy of those features for the participants' sense of context, any formulation is in effect a construction. This is the reflexive corollary of the focus upon practical reasoning. Furthermore, there is a sense in which it is impossible to analyse texts - which can be taken to include recordings or transcripts of interactions - without moving them to the fresh context of the researcher's text. Derrida argues that this phenomenon of citationality is a condition of possibility of language, and presents this

---

[11] It is more fundamental in the sense that, for our purposes, participants' uncertainties are only rendered visible through the researcher's representational work.

as one reason for the impossibility of attempts to define and fix meaning in relation to an 'exhaustively determinable' context (Derrida, 1982:327).

Therefore, whilst it is argued that action and reasoning should be analysed in relation to the setting in which they take place, the sense of what constitutes that setting or context is a matter for interpretation, for participant or researcher. In this respect, claims that a contextual approach to researching human-computer interaction, however formulated, assures a more certain and empirically grounded account need some qualification. Similarly, attempts to codify context as a definable, quantifiable and controllable set of variables in order to bring it within the scope of a system design methodology are untenable. The discussion which follows is intended to illuminate the problems of such claims and approaches, whilst also reaffirming the importance of some version of context.

5.3.2. An empirical instance of context construction

The material discussed in this section is taken from 2.1 in which the user (U) is being shown by her colleague (C) how to produce a short and simple document using Microsoft Word 4 on a Macintosh; she is an experienced computer user, but has rarely used this machine and never used this software. The interaction was not set up by, nor for the benefit of, the researcher but was relatively naturally occurring: it would have taken place when it did, where it did, with the same participants, if I had not been present. Clearly, the intrusion of a researcher and a video camera may have had a significant impact on whatever would have taken place; furthermore I would not wish to claim that this situation allowed more direct access to some pre-interpretative level of empirical fact. However, the non-experimental nature of the interaction was significant in that the nature of the task was not specified by the researcher, and the sense of what exactly they were trying to achieve was therefore available for negotiation between user and colleague[12].

---

[12] This is in accord with Lave's assertion of the value of studying the formulation, as well as the resolution of problems (Lave, 1988).

The participants' utterances produce and sustain a sense of context, a sense that is continually negotiated and unsettled. Understood in this way as an interactional and discursive construct, context turns out to be a crucial factor in shaping both the manner of use and the user's interpretation of the system.

The unsettled sense of context is suggested by the difficulty of giving a simple unitary description of the situation: what is the nature and purpose of what is taking place? The reading proposed here is that there are two senses implicit in the participants' speech: to teach the 'correct' use of Microsoft Word 4 on a Macintosh, or to produce the required document in the most simple and straightforward manner. C, who is an editor of academic materials, appears to subscribe to the former view, and has a clear sense of what constitutes 'correct' practice, whilst the user's utterances can be read as implicitly formulating the latter. C's formulation tends to predominate throughout most of the interaction. Since the sense of what is, and should be, going on can be seen as contested, negotiated and always potentially available for redefinition by the participants themselves, its specification by the researcher must be a matter of considerable interpretative flexibility. I would stress, for example, that whilst I suggest that the material presented below supports my argument, I would not claim that it provides evidence for, proves or grounds the interpretation that I am putting on it; rather, I claim the plausibility of the reading.

'Correct' practice as defined by C implies firstly the maintenance of a clear distinction between the overall style of the document, as specified via the 'define styles' option on the 'format' menu (accessed by mouse or by command t), and its incidental features, which are directly formatted. Wherever possible, the formal definition of style is preferred. For example, in the first few minutes of the session, the following exchange occurs:

(2.1: 2)

U: Do I do the font business later then?

C: No, I think we have to do that first of all doing it properly, so go to command t

The insistence that all aspects of style definition are done 'properly' results in a laborious process of moving back and forth between writing space and style definition areas: 25 minutes elapse before the body text is entered. The final document was very short and relatively simple, and there is no question that the use of direct formatting would have cut the time needed substantially. A further example highlights more clearly the participants' divergent views; this exchange takes place some 20 minutes into the session, after defining an appropriate style and entering a sub-heading:

(2.1: 6)

C: We didn't actually ..

U: We didn't specify space

C: We didn't specify space for that title, so ..

U: Can't I just ..

C: I would say probably let's go back to command t for..

U: Do I need to specify space or can I just sort of do a return to give a blank line?

C: It's bad form, bad form.

U: Is it?

C Yes

U: Seems much easier to me .. (laughs) .. Right, command t

One aspect of this normative judgement of 'bad form' which is worth noting is that the notion of the correct way is not explicated within the software itself in this instance, though it is certainly not unique to this individual. Without wishing to speculate on its precise origins, it is an evident feature of the general occupational culture of experienced Macintosh users at the Open University, where the constraints of the collaborative production of long documents, and the need to maintain consistency, imply a particular approach to the use of Microsoft Word; and perhaps more specifically, of the occupational culture of editors at the Open University. Here we have an instance of the relevance of wider cultural and linguistic frameworks to the construction of context. This is a loose formulation, since such 'frameworks' can no more be treated

as empirically given than can context, but for our purposes it adequately conveys the association of a language game with an occupational community[13].

A second feature of correct practice which has significant effects on the interaction, is the preference for command key use. After being told to save the document at the outset by the use of command s, the user asks:

```
(2.1: 1)
U:    Could I have clicked on save?
C:    You could have done but we try to be keyboard orientated in this
      {manner}
```

The use of 'we' appears to support the plausibility of relating C's views on correct procedure to some kind of occupational community, although this cannot be substantiated. More certain is the effect that this insistence has on the user's interpretation of the system. She continually tries to construe the mnemonic logic behind the use of particular keys, but this turns out to be difficult. For instance, to get to 'paragraph' on the 'format' menu, she is told:

```
(2.1: 4)
C:    To do that we have to do command m .. m for mother
U:    I don't see the logic of m's and d's and things
C:    Well it's funny that isn't it
```

Another setting in which the mouse was used more might produce a completely different sense of the logical character of the interface.

The sense of context which prevails - that the main purpose of the session is to teach the use of Microsoft Word - therefore turns out to have profound consequences for the character of the

---

[13] The inter-textual relations between programs discussed earlier form another such framework.

interaction, since it results in the importation of a series of ideas and procedures that are related to a particular notion of correct practice. However, it should be apparent that this sense of context is largely the participants' discursive creation, and that in no sense do the empirical specifics of the setting have a determinate influence on what takes place; furthermore, the contingent nature of any representation of context, including the one presented here, mitigate against its precise specification in any kind of predictive model of human-computer interaction (cf Barnard and Harrison, 1989). Some of the implications of this formulation are further spelt out in the conclusion.

5.4. The discursive construction of the user as subject

The transcript from which the data discussed in the previous section were extracted is suggestive of a further way in which the theoretical approach to language adopted here could inform the analysis of interactions with and in the presence of the computer: that is, of studying the user as a discursive construct, in the sense that she occupies a subject position that is created for her in discourse. This is to take a step further than the analysis to this point, for the assumption has been that, even where the relations between software have been analysed as textual ones and therefore as significant for the meanings that they take on, that this is at best a mediating factor between software and the meaning that the user gives to it. The user is thus formulated as a stable entity who is the ultimate arbiter of received significations and as such is inherently unaffected by any kind of textual/social/technological interaction.

This is an assumption that remains unchallenged in a number of approaches to the use of technology. For example, whilst it underpins the overtly rationalist approach to human reasoning that predominates in HCI, it is also shared by many of its critics. Thus, as indicated in the previous chapter, Suchman's critique (Suchman, 1987) takes as its focal point pre-constituted individuals and their interaction with a computerised artifact in order to argue not with this concept of individual but with certain characterizations of how such an individual reasons. More specifically, she does not challenge the idea that individuals can be clearly identified as having certain goals - this may be a limitation of her specifying a test for subjects to carry out -

but only that they go about achieving such goals by drawing up and following plans. Similarly, Collins and Yearley's objection to the ascription of any autonomy to artifacts within a sociological account derives in part from a human-centred approach to the construction of meaning which neglects, or denies ,the relative autonomy of texts and discourse (Collins and Yearley, 1990).

To briefly reiterate and clarify the theoretical approach being taken here, the general formulation that the speaking (but perhaps also writing, even directly manipulating) subject be viewed as a discursive construct is a feature of much continental philosophy: for example, part of Derrida's critique of the conception of consciousness as the full presence of meaning prior to the supposed distortions of language rests on the assertion that the subject itself is 'inscribed in language, is a "function" of language' (Derrida, 1982: 15). However it is Foucault's work on the way in which the subject is constructed by specific discourses that is the most relevant here (Foucault 1977; 1979). However, whereas these ideas are used elsewhere in the thesis to examine the constitutive effects of the discourses of particular disciplines, here their implications for the 'micro' study of interactions are considered. Both Silverman (1987) and McHoul (1986) have incorporated the idea of the discursive constitution of subjects within a broadly ethnomethodological analysis, suggesting that speaking subjects are an effect of discourse insofar as the latter provides certain subject positions. To speak of the constitution of the user as subject therefore implies more than his or her representation in someone else's description: it suggests that the ways of speaking that are available to him or her are formed by the discourse in which he or she takes part.

The interaction discussed in the last section again provides the empirical material. The fact that C's formulation of the need for U to learn the 'correct ' methods of how to use the software prevails over any other formulation of what may be taking place suggests the analytic utility of some notion of discursive power. That is not to say that power can be imported into the analysis as an *a priori* variable, by arguing for the relevance of C being male, or of his being in a position of occupational seniority. Other social variables could equally be put forward as relevant: for example, the interaction takes place between friends, involves the production of a document for a

leisure activity - amateur dramatics - in which they are both involved and takes place out of office hours. The discursive positions that each speaker occupies are therefore by no means given prior to the interaction; that they are in general positioned as instructor and learner is the discursive effect of C's insistence on correct practice, a practice about which only he is knowledgeable[14]. The technical character of the software, and C's esoteric knowledge of it therefore provide resources which are used to constitute U as novice in need of instruction. U's utterances can therefore be understood as being framed within this subject position.

I am going to discuss this constitution of the user as subject mainly by looking at some of the frequent references that can be found in the transcript to what either U, or both U and C, are said to 'want'. Such references can be seen as integral to the processes by which U becomes discursively positioned; furthermore, the use of 'want' as an exemplar here has a clear polemical value in contesting both the position that language is the expression of an individual's inner thoughts, and the not unrelated assertion that individuals come to the computer with identifiable goals which they wish to achieve[15].

The use of assertions about what the user wants can be related to two broad areas of interpretative flexibility. Firstly, given that that C belongs to the same theatre group for which the document is being produced, there is some vagueness as to whether he has a proprietorial interest in it; hence it is possible for C to make assertions about what 'we' want to some degree. Secondly, there is no hard and fast division between general design decisions on the layout of the document and knowledge of the specific technical means of achieving them[16]; U's

---

[14] I am indebted to John Bowers for this suggestion.

[15] Both Althusser (1971) and Foucault (1977) claim that the individual's sense of being the autonomous source of their own speech is itself a discursive effect, though the discourses in question are different: for Althusser, the ideological discourse of the state, for Foucault, the disciplinary discourses of the human sciences.

[16] The assumption that this is a feasible separation is a central feature of task analysis : see for example Sebillotte (1988), Card et al (1983) and Kieras and Polson (1985).

ignorance of the latter can therefore be exploited in order to make assertions about her design preferences, about what 'you' want. U's subject position is constructed in relation to the (for her unknown) capacities of the software, and her requirements can therefore be articulated with more certainty by C.

The following two extracts illustrate both these aspects of the use of 'want'. They are inter-related in that although they occur some minutes apart, the second implicitly refers back to the first.

(2.1: 1)

C: Now we probably want title don't we? {}

U: Umm .. do we ? {} it's going to be on the back of this. You mean a title like '3 one act plays in the Barn Theatre', that sort of thing?

C: Whatever

U: I don't think we do. It's going to be on the back of ..

C: Alright, so nothing

U: {} the thing

C: So all you want is your normal style

(2.2: 3)

U: Yeah, and then I want, then I'll do return, then I'll need to do the other business

C: Yes

U: which I want to do in bold, but not big bold, then I want to do my text in sort of, the standard type text

C: So in other words you do want some other styles apart from normal

U: I .. I want (laughs)

C's first utterance 'Now we probably want title don't we?'is taken by U to be a general statement about the intended design of the document, and her response accordingly addresses the question of whether the document needs an overall title. However, C's final utterances in both extracts reformulate it as a question about style definition within Microsoft Word: is it necessary to define any other style apart from 'normal'? For C, there may be no distinction between these two senses, but for U, they are quite separate. Thus her response, which was addressed purely to the question of the layout of the document, is taken to mean that no other style should be defined (and note that at this early stage style definition within Microsoft Word is an unknown area to her), whilst her subsequent comments in the second extract on stylistic features can then be taken as contradicting this : 'so in other words you do want .. '. This elision of a technical discourse and a general discussion on design features produces what Silverman calls an 'interactional trap' (Silverman, 1987: 166) for U: her final utterance in the second extract can be read in terms of the difficulty of resolving this elision from within the subject position that has been created. This interactional problem is also evident in the following:

(2.2: 6)

C: Now we don't want any more space below probably but umm I would say

U: No I want space underneath it before the text bit

C: I mean no sorry we don't want any more space above because we've already got the space below incorporated within the, the title, so go to before

Here, C's initial assertion that no more space is needed below the title is directly contested by U: she does want space in between title and text. Again this is formulated as a straightforward design decision, made by the person ostensibly responsible for such decisions. The final utterance however resolves the conflict by reformulating the intended sense of the first statement as technical: should (more) space be defined above the text since some has already been defined below the heading? In the process, the legitimacy of U's response is challenged: it is reframed as a misunderstanding, even though C's apology concedes some of the responsibility for this. U's

attempt to speak from the subject position of author is thereby subverted, and she is re-constituted as novice in relation to the technicalities of the software. Once again, the first utterance, which U is constructed as having misunderstood, does not distinguish between the appearance of the document and the technical means of achieving it, and it is this lack of distinction that both enables C to involve U in discussions about the form of the document, and serves as a resource for constituting her as novice.

It is then a matter of some difficulty for U to articulate preferences for the form of the document without having her authority for doing so challenged, even though her position as putative author and designer are frequently acknowledged. Nearly all the uses of 'want' are made by C, speaking on behalf of either her or them both, whilst U's attempts in this direction can always be undermined with reference to technical issues and her ignorance of them. The following extract is particularly telling in this respect:

(2.1: 2)

U: Was this, this zero here, that's still going to, what's the indent

C: Oh, that's automatic

U: And how do I know that's what I want? (laughs)

The final utterance, broadly stated, questions her own ability to specify her requirements because she is unaware of the relationship between the technical definition of style and the appearance of the document. There are many ways in which this utterance could be more precisely read; for example, an approach that would be broadly in line with the prevailing orthodoxy of task analysis would be that she has a clear idea of what she wants in terms of the preferred design of the document, but doesn't know how to specify the technical means to achieve this. In this reading, the idea of an individual who brings a set of pre-defined goals to the computer remains unchallenged, whilst 'want' is seen as an invariable signifier of such goals. But a more interesting reading , which takes note of the flexible meaning of words on different occasions of use, observes that the logical grammar of this sentence (Coulter, 1983) suggests that it may be

possible to 'want' something but be simultaneously unaware of what that something is. Here 'want' is used in the sense of requirements which need have nothing to do with conscious wishes. This flexibility forms yet another resource which can be used to position U as other than the author and designer of the document, since C can quite legitimately claim to know her requirements better than she.

I have suggested that the user is constituted as a subject through the discursive interaction between her and her colleague, and that the subject position that is created for her is one from which it is difficult to articulate preferences, given the elision of technical and more general design issues in C's speech. It is therefore C who is able to articulate preferences on her behalf. Although, as we have seen, she does attempt to contest C's representations in this respect, in general she accepts their legitimacy. ('I'll bow to your greater knowledge of these things' (2.1: 7)).

Let us consider some of the implications of this analysis. Firstly as has already been noted, it challenges the view of the individual that prevails within HCI, including the work of ethnomethodological contributors, in that it sees the speaking individual as located within and formed by discourse. It therefore calls into question the idea that utterances can be used as a 'protocol' to shed light on the user's reasoning processes[17]; it also questions the viability of purportedly 'user-centred' approaches to HCI (Norman and Draper, 1986), in that it rejects the latter's implicit appeal to the user as pre-defined entity prior to interaction. However, in another respect it is itself human-centred: in acknowledging the importance of the social interaction around the computer for the very existence of the discourse that takes place, and in relating utterances to that interaction, it necessarily marginalizes the possible role which the computer might play in the analysis. The subject may be constituted by discourse, but discourse is simply taken as that which arises in the interactions between people. In a sense, the sociologist in this domain faces a dilemma here. I have noted that Suchman's approach is to marginalize the significance of the interactions between people in order that users' discourse may be more

---

[17] A typical example of the use of this approach in HCI can be found in Carroll and Mack (1985).

plausibly related to the ways in which they interact with, and make sense of the technological artifact[18]. In order to maintain this focus on the interaction of user and machine, a version of the individual is presented as prior to the contingencies of social interaction, and in this respect at least, consonant with the individual of cognitive science. By contrast, the approach that I have taken which stresses the discursively constructed, and therefore socially constructed character of the individual, ends up by minimizing the significance of the computer for the analysis of discourse which takes place around it. The 'humanist' approach to the theorization of the subject therefore admits the machine to the analysis, whilst the 'decentred' approach pursued here appears to exclude it.

It may be that this is not a genuine paradox, but merely reflects a deficiency of my analysis. A way of maintaining the theoretical stance that I have taken but of avoiding the exclusion of the machine is to follow through some of the implications of considering software as analogous to discourse, as outlined in an earlier section[19]. How plausible is it to consider the process whereby the user is constituted by the software? The only significance I have granted to the software has been relatively indirect, stressing rather C's esoteric knowledge of its technicalities as relevant to the constitution of the user in this interaction. However, the focus has been exclusively on spoken discourse and the speaking subject. If we were to consider the user as a writing subject, or perhaps an 'inputting'subject, then the role of the software would be more central. In these terms, software constrains and defines the subject position of the user in terms of the actions they may legitimately make, the parameters of valid input at any point. At the same time, I have argued that the user's encounter with the software can be mediated by the interactions which takes place around the machine; notably, in this case, through C's sense of the importance of learning

---

[18] 'The actions of the team members can also be viewed as organized by the task of collaboration itself, although in the interest of looking at the interaction of both users with the machine, I have deliberately avoided taking that view here' (Suchman, 1987: 115).

[19] I should stress that my objection is not so much based on an *a priori* commitment to include the machine within the scope of a sociological analysis, but on a wish to avoid its exclusion by default as a consequence of the method of analysis adopted.

the 'correct' way to use the software. Acknowledgement of the possible relevance of both these dimensions raises a number of theoretical questions. For instance, is it legitimate to theorize the user as simultaneously occupying two subject positions, with respect to software and to spoken interaction around the computer? Or rather, what would be gained by such a model? One problem with suggesting that a user's input to a computer is constrained by the subject position created for him or her by the technical parameters of the software is that it tells us something totally unsurprising, and simply clothes mundane experience in theoretical terminology[20]; by contrast, the notion that the speaking subject is a creation of discourse challenges fundamental and entrenched conceptions of language and individuality.

In a sense, this question brings us back full circle to some of the problems of making a distinctive sociological contribution to the study of computer use that were discussed in chapter 2. Observing and noting the specific details of computer input and output includes the machine in the analysis, but places the analysis on an uncertain terrain. As Joerges (1990) concludes in his summary of images of technology in sociology, in order to move beyond accounts of machines to study technology as a body of silent practices, 'we have to invent a language for talking about those practices sociologically' (Joerges, 1990: 227)[21].

---

[20] My implicit position here, that sociology should aim to offer a distinctive and novel view of the world, is at odds with some formulations of ethnomethodology. It may be an exemplification of what Sharrock and Anderson have called the sociologist's 'mania for changing the subject' (Sharrock and Anderson (1979: 8). Whilst I share their view that attempts to reveal truer, deeper meanings beneath mundane experience are misconstrued, I do not follow the implicit direction of their argument that this rules out the possibility, or desirability, of offering a *different* perspective on social action: McHoul's response demonstrates clearly the possibility of such an account which would not fall foul of their justified suspicion of 'over-ironising moves in sociology' (McHoul, 1984: 69).

[21] Such an invention is beyond the scope of this thesis. Latour (1988a) suggests a way of reading artefacts as texts: how far it could be generalized to include artifacts such as computers is uncertain.

5.5. Conclusion: some implications for HCI and sociology

5.5.1. Interaction and discourse

The central resource for the study of computer use presented in this chapter has been discourse: chiefly, the speech that takes place around the computer between people, and to a lesser extent, the consideration of software as discourse. (The possible implications of the latter will be left to one side for the moment). This approach can be justified by pointing to the interest of such discourse as a constituent feature of these settings, and, perhaps more pragmatically, to the problems of dispensing with this fundamental analytical resource and attempting to produce a meaningful sociological description of silent interactions with computers. However, since discourse is treated as a reality in its own right and not as an index of some other reality, there is a sense in which its relation to human-machine interaction will always be an oblique one. This is in contrast to the use of discourse as a protocol which may be used to shed light on the reasoning of the individual in the course of his or her use of the computer. The emphasis on the socially orientated character of discourse is at odds with such a position[22].

This oblique relation of the spoken interaction around the computer to the interaction with the computer can be construed as a problem for its relevance to HCI insofar as it ends up, as Payne suggests is typical of most work in the field, neglecting the interaction itself (Payne, 1990). It could be argued in response that the formulation of computer use as 'interaction' begs as many questions as it answers since, as Anderson (1986) and Kammersgaard(1988) have argued, a number of different 'perspectives' are possible on the relation between user and computer, each carrying with it particular connotations. The theoretical position adopted here suggests that it would be misguided to try and adjudicate on which description best matched some formulation of the 'essential' reality of computer use. Therefore the problem that I would concede for much of the analysis presented here is not that it fails to do justice to interactions between human and

---

[22] Goffman suggests for example that even such apparently self directed talk such as 'oops', or equivalent forms blurted out in the course of solitary activity, should be understood as a form of public display to those who are there to overhear it (Goffman, 1981: 100).

computer, but that it fails, in certain respects, to keep open the possibility of formulating such a description. In so doing, it runs the risk of unnecessarily circumscribing the limits of the social by restricting interaction to that which occurs between people.

Against this however, the analysis of the spoken interaction around the computer is of direct relevance to the understanding of the (possible) phenomenon of human-computer interaction in a number of ways. Firstly, as I have already indicated, such discourse is in Garfinkel's terms, a constituent feature of the setting it makes observable (Garfinkel, 1967: 8). It is not therefore peripheral to the 'real' business of computer use: indeed in one setting in particular, we noted the pervasive effects of talk on the way in which the software was used[23]. But the postulation of effects is not necessary to establish the relevance of talk: talk around the computer is part and parcel of the cultural phenomenon of computer use, and it is in this that its relevance lies.

Secondly, it is instructive to look at the ways in which people formulate their relation to the computer: that is to examine 'interaction' as it is discursively constructed in situations of computer use. This has relevance for two areas of debate. Within sociology, arguments and discussions on the limits of the social in relation to technological artefacts turn crucially upon the granting of agency to machines, whether by sociologists or social members more generally (Collins and Yearley, 1990; Latour 1988a; Woolgar, 1989). Within HCI, the viability of a notion of interaction with machines has also been raised (Bench-Capon and McEnery, 1989, Barlow et al, 1989). Some of the material examined here suggests not only that people mundanely grant agency to machines and thereby presumably give tacit support to some notion of 'interaction', but that analysis of the detail of this attributional work reveals some altogether more complex formulations: distinctions are made between elements of the software, agency is differentially

---

[23] A noticeable feature of all the data discussed in this section is what Goffman has called the 'open state' of conversation which accompanies an instrumental task (Goffman, 1981: 135): that is, the absence of the same obligations, in terms of immediate responses, that one would expect to find in other forms of talk. There is also considerable scope for looking at the frequently rhythmical way in which talk can be interleaved with actions during instruction (cf Auer, 1990; Button, 1990b).

granted to some of these elements, and the user frequently identifies him or herself with the behaviour of some of them. In comparison with the ways that people talk about their use of computers, debates on agency and interaction seem (already) somewhat too black and white.

Thirdly and finally, the interpretation that has been offered here suggests that discussions on interaction and agency, and indeed nearly all work within HCI, operates with a concept of the user as pre-constituted subject, disagreements about the nature of that subject and its reasoning notwithstanding[24]. I have suggested a different view of the subject as constituted in the course of discursive interaction (although I have largely restricted the terms of my analysis to spoken discourse). Such a view not only reinforces scepticism about the use of verbal protocols to gain insight into the user's reasoning (practical or cognitive), but also questions the terms of HCI's calls for more 'user-centred' design[25]. Moreover, it may have important implications for attempts to classify users.

5.5.2. Classifying users

Within computer science in general, there is a recognition of the diversity of users and of the need to understand this diversity in order to design suitable systems. Thus for example, Everest constructs a typology of database users based primarily on organisational roles, likely tasks, and the degree of skill required (Everest, 1986: 74-84). Another approach, from within HCI, is to attempt the specification of cognitive differences between 'novices' and 'experts' (Mayer,

---

[24] Woolgar (1990) is an exception, but even here most of the discussion of the 'configuration' of the user concerns his or her representation by others, notably computer professionals. The construction of subject positions however is not restricted to questions of representation, since its analysis also posits the significance of what discourse *does*.

[25] 'User-centred' design, and the general project of more accurately representing the user (Suchman, 1987, Whiteside et al, 1988) are of course further called into question if one adopts, as here, a sceptical view of language: that is, one denies the validity of positing an essential user prior to the various descriptions of him or her that may be offered.

1988)[26]. These classifications are relatively rigid. By contrast, the material from data 2.1, discussed above, suggests that novice and expert are interpretatively flexible categories. The precise relevance of the different dimensions of computer experience that each participant possesses is not given at the outset; that the user is placed firmly in the position of novice in need of instruction is an effect of discourse. Whilst her colleague's technical knowledge of this particular software means that she will require some assistance from him, the learner/instructor relationship that is apparent in this material is not an inevitable consequence of this. Technical knowledge is used as a resource in the process of constituting her within a subject position in which the significance of both her previous computing experience and her role as putative author of the document being produced is marginalised. Therefore, whilst novice and expert are generally thought of purely in terms of the acquired levels of knowledge and skill within each individual, in this instance they can be understood as, in large measure, social roles which are created in the course of social interaction.

5.5.3. Social context

I have argued that whilst 'context' has considerable value as an analytic concept, it is not to be taken as either determinable or determinate in any simple sense, since if we follow the sceptical and deconstructive implications of ethnomethodological thought, context should rather be viewed as construct and resource for participants and researcher. This tends to be overlooked not only in HCI but also in ethnomethodology itself. The reading of the context of the interaction in data 2.1 therefore has to be taken as both an account of the construction of context and as an example of it, with all the uncertainty that that implies[27].

The relevance of this for HCI is twofold. Firstly, it circumscribes some of the claims that are made for contextual research, especially insofar as they assert a more direct and unmediated

---

[26] See also the specification of cognitive characteristics in the experiment discussed in chapter six.

[27] One paradoxical consequence of the reading's reflexive character is that its credibility as account may stand in an inverse relationship to its credibility as example. For a detailed analysis of the some of paradoxes of reflexivity, see Ashmore (1989).

access to empirical reality by virtue of more 'naturalistic' data gathering techniques. Secondly, it has practical implications for attempts to incorporate contextual details into global models of human-computer interaction. This is only tenable if context is seen as posing an empirical problem of quantity, insofar as the study of interactions in context reveals a number of variations from situation to situation. Barnard and Harrison (1989), for example, propose the possibility of a model, which would comprise not only a system model and a cognitive model of the user, but also an interaction model which would incorporate 'characterisations of interactional states'. These characterisations could capture keystroke level analysis or 'analysis of situated actions', 'depending on the grain of task analysis required' (Barnard and Harrison 1989:100). There are a number of problems with such a formulation. In the first place, the incorporation of Suchman's work into the category of 'task analysis' is highly questionable, since it is explicitly critical of a basic assumption which underlies most variants of the latter: namely that user activity, conceptualized in terms of tasks, can be decomposed into hierarchies of plans and goals[28]. Moreover, one implication of my analysis is that the representation of a 'task' may not exist prior to and apart from the interaction, but can be understood as part of the negotiation and interpretation of context that takes place during the course of the interaction (and even as part of the process whereby the user is constituted as a subject in discourse). More fundamentally, the project would only be feasible if a drastically impoverished version of 'situated actions' were employed. The essential problem is not that contextual variation is so great that it cannot be completely codified, but that the meaning and significance of context and its features is not fixed or empirically given: it is not only an interactional resource created by participants, but is also subject to interpretative flexibility at all levels[29]. Even if context is strictly delimited to the temporal sequencing of input and output, this remains the case. As such, its use within system design methodologies which aim to predict and model user behaviour is misconceived. Rather,

---

[28] See for example Sebillotte (1988), Card et al (1983) and Kieras and Polson (1985).

[29] 'If totalization no longer has any meaning, it is not because the infiniteness of the field cannot be covered by a finite glance or a finite discourse, but because the nature of the field - that is, language and a finite language - excludes totalization. This field is in effect that of play, that is to say a field of infinite substitutions ..' (Derrida,1978:289).

the phenomenon of context poses a radical challenge to predictive methodologies of this sort: attempting to incorporate it within them will not remove that challenge.

5.5.4. Software, discourse and the limits of the social

I have affirmed the utility of relating the perception and interpretation of software to the social context of its use, albeit in theoretical terms which, in pointing to the necessary flexibility of any formulation of context, are at odds with the way in which the concept is more usually employed. However, I have also suggested that the external relations within which software takes its meanings must not be restricted to the immediate situation of its use, but to, notably, other pieces of software and in some cases the conventions that are associated with them. In so doing, I am suggesting an analogy between software and discourse: like a text, a program has relative autonomy with respect to both the intentions of its author(s)[30], and, to a considerably lesser extent, the interpretative work that is done by its users. This analogy represents the application of a relatively uncontroversial theory of textual meaning to the domain of technological artifacts. If the analogy is a reasonable one, then computer software falls within the domain of the social, or more pertinently of the sociologist, in the same way as do written texts.

Part of the recent debate within SSK on the granting of agency to artifacts turns on precisely this question. Whilst claiming agency for artifacts may entail more than stating their equivalence to texts (Latour, 1988a: 308-9), it is interesting to note that the major critique of this approach takes issue not with the extension of texts to machines but with the granting of autonomy or authority to texts in the first place (Collins and Yearley, 1990: 14-16): the critical question of the relation of texts to artifacts is therefore neglected. As the authors acknowledge, their approach is 'human-centred' (ibid: 13). Perhaps it is so to an unusual extent, in that not only does it

---

[30] Bench-Capon and McEnery 's argument (1989) that computer use should be viewed as the interaction of people *through* computers, ie. that the user interprets programs by trying to deduce the designer's intentions, is significantly at odds with this approach, even though they urge the value of considering programs as texts.

confidently propose distinctions between humans and artifacts, independent of and apparently prior to their representation (ibid: 26; see also Collins, 1989: 621), but also in that it denies *any* autonomy to texts and textual inter-relations; the meaning and authority of texts, inscriptions and other representations derive purely from the interpretative work of the human subject.

Ultimately, the validity of affording a more central and active role to artifacts in sociological accounts must be judged, as Collins and Yearley themselves assert, in terms of its explanatory power, and not on the basis of *a priori* judgements. This chapter has attempted to avoid such an *a priori* exclusion insofar as the account of computer use that it offers is in important senses not human-centred, being formulated within the theoretical orientation to language which informs the thesis as a whole. In so doing it contrasts not only with the cognitive mainstream of HCI, but also with existing sociological work within the field, and reinforces my claim that such work is, in some respects, limited in its scope.

Chapter 6

Experiment, user, design

In this and the following chapter I examine some empirical instances of HCI practice. I have argued for a conception of discourse which suggests both that disciplinary formation entails a process of establishing a distinctive discourse, and that the speaking subject is always located within discourse. The process of discursive formation is the focus of chapter seven; here, I am concerned with the significance of the discourse of HCI, as it currently stands, for practitioners' work and utterances. By looking at HCI on the ground, I aim to show the significance of the parameters of HCI discourse, as discussed in chapter three, for the form of particular statements and practices, and to shed more light on some of the central features and assumptions of the discourse itself.

The primary source materials for the chapter are taken from the study of some aspects of a project funded by the National Physical Laboratory called 'Adaptive Systems and User Modelling Tools'. A central part of this project was an experiment which was designed to see if there was any correlation between performance on different 'types' of interface, and performance in tests which measured specified cognitive characteristics. Some ethnographic observations are offered on the experiment and on the subsequent analysis of its results; but the central resource is a consultation which took place prior to the experiment, in which each of the interfaces of the prototype system was evaluated in terms of how representative it was of a given interface type, and a report produced. As part of this consultation, the system/experiment designer and the

consultant spent some time at the computer examining, using and talking about the system: this was videotaped and transcribed (Data 2.2). Like the material examined in the previous chapter, this is a human-computer interaction; however, it is HCI practice and discourse, as evidenced in what takes place around the computer that is the point of interest, and to this extent the interaction with the system is incidental to the analysis offered here. Material from other instances of HCI practice is used to support and amplify some of the points which are suggested by this data: a meeting of a committee formed to draw up standards for icons and graphical symbols (2.3), the exclusive focus of chapter seven, and a tape recorded panel meeting from the Interact '90 conference on 'New approaches to theory in HCI: how should we judge their acceptability?' (2.4).

It is important to be clear about the relationship between this chapter and the analysis of HCI discourse presented in chapter three, which was based on published materials. It is not the intention here to contrast the public face of HCI, as evidenced in written papers, with what 'really' goes on in its mundane practice: this much should be clear from the theoretical position I have outlined, and from the direction that the argument has taken. Rather, I wish to show how both texts and talk/practice are framed within the same discursive structure. For example, much of the interest of the consultation process derives from the requirement that the system be evaluated by an HCI expert: it is therefore incumbent on the consultant to ground her evaluation on established HCI terrain, to make her report a recognizably HCI piece of work, and to occupy the subject position of HCI practitioner with all that that implies. The established structure of HCI discourse therefore has considerable significance here. In this instance, an ethnomethodological reading of the relation between the wider discourse and the consultant's utterances suggests itself: that is, that she consciously places herself in the subject position of HCI expert in the course of making the evaluation required. Such a reading has some validity, and has the merit of pointing to the ways in which speaking subjects can manipulate and use different discourses: in other words, that the relation of discourse to subject is not a rigidly determinate one. Indeed, there is sufficient flexibility about the purpose of the consultation to ensure that different subject positions are available to, and taken up by both speakers in the course of the interaction: the consultant speaking both as adviser on interface typologies and as

someone with experience in cognitive psychology and experiment design, and her colleague as experimental designer and software engineer. However, as I have argued throughout, I do not think the relation between discourse and utterances can be reduced to this model of conscious usage, since to do so implies that language is simply the expression of the speaker's prior thoughts: rather, the speaking subject is always located within a discourse, or discourses, and it is therefore the discourse of HCI that is the point of interest in what follows.

6.1. An HCI experiment

6.1.1. Experimental design

First of all, I wish to draw attention to some central aspects of the experiment and its design; some of these can be directly related to some of the key features of HCI discourse as identified in chapter three: for example, to the stress on efficiency in discussions of usability and the frequently tacit identification of both.

The experiment involves the use of an exemplar system which has several different interfaces, and is aimed at 'identifying and isolating cognitive characteristics of users which are real predictors of user performance on the various interfaces' (Benyon et al, 1990: 573). The more general aim of the project is to move towards the realisation of systems which could adapt themselves to different users, that is, change the user interface to suit the cognitive characteristics of a given user, and to build software tools to facilitate the development of such systems. The feasibility of this general goal is difficult to assess, and the researchers recognize that 'considerable work needs to be done in this area in order to determine the relationship between cognitive characteristics and interaction' (ibid: 575): the experiment is a first step towards the specification of that relationship. It should be apparent that the project is premised on the separation of user interface and system functionality which is central to HCI discourse.

The experiment comprised two parts, performance on a set of specified tasks on each of five different interfaces, and on a series of tests away from the computer designed to measure cognitive abilities and personality types. In addition, each subject completed a brief questionnaire on the extent of their experience using computers. Since levels of performance on each interface were to be compared, a problem that arose in the course of designing the experiment concerned the question of equivalent difficulty. This was raised at a meeting prior to the consultation, which was attended by the experiment/software designer, a project director, the consultant and myself. Specifically, the perceived problem was that the 'command' interface, in its then current state of implementation, might be disproportionately difficult. This was countered by the project director with the assertion 'but they are hard': the implication being that some types of interfaces are inherently more difficult to use than others. Accordingly, the emphasis was placed on making sure that each interface was representative of its given type rather than on trying to establish equivalent levels of difficulty; however, the consultant's report suggests that part of her brief is to 'ensure that the interfaces have been designed to be equally optimised examples of that particular category' (2.2, Document 1: 4), although she notes that such an assessment would be very difficult to achieve in any objective sense. No attempt is therefore made to establish equivalent levels of difficulty, but it is notable that this is seen as defeasible primarily in terms of problems of measurement; the viability of talking about 'difficulty' as an entity that exists independently from the situations of its occurrence, in a Platonic sense, is not called into question. As far the consultation is concerned, the key issue in the evaluation is therefore not the relative difficulty of each interface but its typicality.

The conception of performance that informs the experiment is based purely on speed; as the consultant states in her report 'the brief given was that the focus of the empirical study was on throughput (ie performance measures of ease-of-use) rather than learnability' (2.2, Document 1: 4). Again the problem of how the analysis should deal with differences and equivalences between interfaces is germane here. In the course of the consultation, the question is raised of whether, or in what way, a system delay on one of the interfaces would be taken into account in comparative measurements. The designer responds as follows:

(2.2: 8)

F: Yes. There's going to be .. the thing is, if you plot for the whole group the times for each interface you'll get the times not taking individual differences into account, and then I mean there's an analysis where you can zero those, make those all the same and look at deviations from, from the mean time for different people. I mean I don't think there's any way I can possibly try to make the interfaces take the same amount of ti

Y: Oh no no

F: average time. So I think the stats is going to have to take care of that ..[1]

The aim of the project is formulated more generally in a number of places, notably in terms of finding out which interfaces 'suit' which people. For example, the designer states during the consultation that she wants to find out 'what sort of interfaces suit which people' (2.2: 17); or again, part of the stated rationale for attempting to build adaptive systems is that adjustments can be made to 'facets of the interface to suit the individual' (Benyon and Murray, 1988: 465). However in the experiment, all that is measured is speed over a number of specified tasks, and deviations from collective norms of performance. In this respect, the experiment exemplifies, in a particularly explicit manner, the tendency to equate usability, or 'ease-of-use', with efficiency as measured in relatively mechanistic terms. It is interesting to note however that whilst this is an explicit part of the experimental design, there are rhetorical contexts in which the mechanistic aspects of the process are downplayed. For instance, when the consultant raises the problem of the stress that subjects may feel, and the possible effects that this may have on their performance, F states:

---

[1] The importance that is granted to statistical techniques, as in this extract, and its relation to conceptions of scientific inquiry that inform the research process is discussed in the following sub-section "Results and their interpretation'.

(2.2: 21)

F: Mm. I mean I'm not going to say you've got to do this as quickly as possible, I mean it'll be more sort of as quickly as you feel comfortable doing it

Here, there is a rhetorical counter to the suggestion that the timing of performances in a laboratory test would be stressful, which emphasizes the reasonableness of the procedure that will be adopted: the definition of performance in terms of efficiency and speed is not contested however.

6.1.2. Results and their interpretation

Of particular interest in the course of my observation of the different stages of the experiment and its analysis was the interpretation of the results. There are two aspects of this process that I want to note: the inferential procedures used, and the conception of scientific inquiry implicit in discussions about the significance of the results. I will examine each in turn.

I have noted that the experiment is designed around two distinct sets of tests, and that it seeks to establish connections between performance on each. However, performance on each set is not treated symmetrically. This can be clearly illustrated with reference to the ambiguous or asymmetrical role of previous experience. In terms of the tests at the computer, previous experience of given types of interfaces are held to be relevant:

(2.2: 17)

Y: Do you think that's going to affect your results, that {} people with experience with

F: Well, this is something we're looking at, I mean I've got a questionnaire I've written about experience with different interfaces that are similar to these, so that's going to be looked at

Previous experience of similar computer interfaces is therefore held to be a legitimately available explanatory variable in interpreting the results: and indeed, in a subsequent experiment it is used to explain (away) anomalies with respect to the claimed correlation between spatial ability and facility in using one of the interfaces. However, previous experience of doing the other set of tests is not held to be a relevant variable: these are tests that are designed to measure the five cognitive characteristics of spatial ability, verbal ability, short-term memory, logical intuitiveness and field-dependence, and three personal profile attributes. Even though this latter set of tests takes place within time constraints, and the intention is to complete as much of them as possible, the results of these tests are not treated as 'performance' in the same way: rather, they are accepted as measures of (presumably unchanging) cognitive characteristics. Consequently, previous experience of these tests is held to be beside the point. The asymmetry is also apparent in that whilst performance in one set of tests can be used to explain performance in another set, this explanatory schema is uni-directional: thus, whilst all that is looked for and claimed are 'correlations' between performance on both tests, this is formulated as being between cognitive 'abilities' and 'performance on the five interfaces' (Benyon et al, 1990: 576). It would not, for instance, be legitimate to formulate ability on button interfaces as an adequate explanation for a good performance on field-dependence tests. Inferences can therefore be seen to rest on a presumed underlying reality of innate, and measurable cognitive characteristics[2]; and furthermore, the techniques which are used to deduce and measure such characteristics are treated as essentially unproblematic. In other words, the experiment is underpinned by both a set of assumptions about cognition, and a realist conception of approaches towards its analysis.

This brings us to the second aspect of the interpretation of the experimental results: the conception of scientific inquiry that is implicit in discussions of the significance of the results. Upon completion of the analysis, the results are spoken of in realist terms as evidence for matters

---

[2] This is another instance of Garfinkel has called 'the documentary method of interpretation' (Garfinkel, 1967: ch 3; see also chapter two of this thesis for a brief resume). In this instance, the arbitrary character of the inferential procedure centres on the issue of what is to be taken as signifier and signified respectively.

of fact that have been uncovered by the experiment: in Gilbert and Mulkay's terms, discussion of their significance draws upon an 'empiricist repertoire' (Gilbert and Mulkay, 1984). Prior to this however, discussions are characterised by both uncertainty as to the likely pay-off of the inquiry, and, moreover, draw upon a 'contingent repertoire' (ibid) that formulates the research as an active process of manipulation of empirical materials. I will briefly outline this progression towards realism. In the consultation prior to the experiment, after F has explained that she is looking for 'what sort of interfaces suit which people' the following exchange occurs:

(2.2: 17)

Y: Do you think there will be a difference?

F: I don't know ..

Such uncertainty is of course a perfectly sensible attitude to take prior to the experiment, avoiding as it does any pre-judgement of the possible results. In practice however, there is always pressure, on any research, to succeed, to produce newsworthy results[3]. Consequently, in the course of the experiment and its subsequent analysis, increasing emphasis is placed on the techniques that can be applied to the data in order to make them yield up their significance. During the few days when the experiment took place, one of the project directors had a brief conversation with the designer about how things were going, and was told that she was unsure about how effective the results would be. His response was that if no statistical significance was found, they could argue for the unsuitability of that method for this type of material, and base their case on consideration of qualitative aspects of the data. This would be a remarkable course to take, given the strictly quantitative parameters of the experiment itself: but I am not concerned to challenge the validity of such an approach, but to point out that the possibility of research which fails to deliver noteworthy results is effectively ruled out in this exchange.

---

[3] This is of course equally true of more discursive forms of research such as 'qualitative' sociology, as exemplified in this thesis, where the requirement to deliver 'news' can be read in the narrative style that is usually adopted.

Analysis of the results of the completed experiment took the form, initially, of trying to find a suitable statistical technique that would show some significant correlation(s) in the data. In the informal context of the designer's office, she was explicit in this respect: that is, she saw herself engaged in an active process of searching for a way of manipulating the data in order that it might be shown to be significant. When a technique was finally found which suggested a significant correlation between a cognitive ability and performance on one of the interfaces, her comment was 'I think I've found a way of making it significant'; the phrase 'making it significant' locating this assertion firmly within a 'contingent repertoire'. However, once such a correlation had been achieved, it was noticeable that the experiment and its results were formulated in markedly realist terms; for example, 'early results *suggest* that the level of spatial ability does correlate significantly with ..' (Benyon et al, 1990: 576, my emphasis). This shift to an 'empiricist repertoire' was not confined to formal, written discourse but also characterised informal speech. It is apparent, then, that the 'final' results are formulated in the realist terms that are characteristic of HCI discourse; but it is also apparent that, as much work in SSK has argued, if we look at the process that precedes the settlement of what are to count as findings, a different way of talking about the research process is used which grants more to the active role of human agency in the production of results.

6.2. Representing the user

It was noted in chapter three that HCI can be characterized in terms of its representation of users in two senses: it constructs cognitive representations of them, and it acts as their representative within the wider enterprise of designing and building software. In this section, I want to show that this representational work is an evident feature of HCI practice, and examine in a little more detail HCI's role as advocate.

6.2.1. The cognitive representation of the user

As will be apparent from the preceding discussion, the representation of the user in cognitive terms is an explicit feature of the experimental design. In this respect, the experiment typifies

the concern with the cognitive that forms part of the core of HCI discourse[4]. Given the explicit nature of the cognitive aspects of this project, to demonstrate the congruence between it and the wider discourse of HCI would be to labour the obvious; rather, I will briefly draw out some of the assumptions that appear to inform the project in this respect.

The motivation behind the very idea of attempting to develop adaptive systems is expressed in individualistic terms. I have noted already that the stated intention is to enable adjustments to be made to 'facets of the interface to suit the individual' (Benyon and Murray, 1988: 465). The emphasis on the individual is derived from a dissatisfaction with the generality of categories such as 'expert' and 'novice' (ibid: 465); a fully implemented adaptive system, it is claimed, would be able to bypass the crudeness of such categories by adjusting itself to the characteristics of the individual using it. In order to do this, an 'embedded user model' which stored information about cognitive and personality characteristics would be incorporated into the system. The task of inferring such characteristics from the details of the interaction is recognized by those involved as a difficult one that needs 'considerable work '; it is hoped that the experimental results will shed some light on the problem by revealing an association between interaction and cognition.

Two implicit facets of this conceptual scheme should be noted. Firstly, users are regarded as individuals who can each be measured in terms of cognitive characteristics[5]. Secondly, and less obviously, there is an assumption that the interaction between computer and user could be significantly improved if the computer could, in some way, be made aware of those characteristics and adapt to accommodate them. Gilbert (1987) has however pointed out that if one considers interactions between humans, this assumption loses much of its intuitive appeal: for

---

[4] By the same token, much that I say in this chapter on the discourse of experiment is applicable to cognitive psychology.

[5] In terms of planned implementation however, the embedded user model would be of classes of users, or stereotypes: modifications to enable a more individual profile would come at a later stage (Benyon et al, 1990: 575).

people converse and interact in sophisticated ways without, in any readily apparent sense, forming cognitive models of each other.

It was noted earlier that the method of interpreting the experimental results, entailing as it does the asymmetrical treatment of performance in the two sets of tests, is premised on an implicit conception of cognitive abilities as innate and unchanging. This issue is in fact explicitly addressed elsewhere, though somewhat equivocally. For example, whilst Benyon and Murray refute the suggestion that they are implying that 'personality and cognitive characteristics are completely non-adaptive', they argue that it is desirable that the system adapts to their 'preferred working styles' (Benyon and Murray, 1988: 467); furthermore that these characteristics are 'relatively stable' (ibid: 468). In terms of the experimental design, this difficulty is dealt with by using subjects with some experience of the various interface types. The following extract sheds some light on the reasoning behind this:

(2.2: 17)

Y: Are you going to be looking at all that, learning rates

F: I'm not interested in learning, just in final performance. I mean obviously

Y: So it's purely use

F: people go on learning to some extent but

Y: you're purely interested in use, ease

F: Yes

Y: ease of use for experienced users

F: Yes, I mean what I want to find is, what sort of interfaces suit which people, sorts of people best, in the end. I mean other {people'd} like, all these training things are dealing with, um, how its best to train people to use that interface, but I mean what I want to find out what interface is best for what people eventually ..

To what extent the strategy of choosing experienced users avoids the complicating effects of the user's adaptations, to use the project's terms, is an open question, although one might wish to question the implication that experienced users can be defined as having reached a stage at which learning has finished. It is fair to say however that the marginalisation of learning in this project is by no means unusual within HCI; there have been very few longitudinal studies of how people learn to use software, and relatively little attention to work from educational psychology on the learning process. The cognitive science models that predominate in the discipline tend to be static ones.

6.2.2. The user's representative: identity and difference

The stance that HCI takes in relation to both user and designer is an unambiguous one. The following extract from a meeting of HCI practitioners engaged in the production of standards for graphical interfaces articulates this position with clarity; the topic under discussion is the optimal form that should be specified for a particular icon, and the criteria that should apply in defining such an icon:

(2.3: 14)

NB: the question is, you, we're trading off sort of ingenuity and elegance of design, flexibility of design against the merits of a standard, and the question is, well do we have a consensus here which {taking} the UK view as to which is more important?

TC: I would say the user's recognition of it is more important than the designer's freedom to exercise his

NB Yes,

TC skills.

The advocacy of the user's point of view is one of the foundational elements of HCI discourse, and, as has been noted already, the position that the discipline takes up in relation to design practice is largely a function of this advocacy. In the consultation that takes place prior to the experiment, such advocacy is apparent. However, given that in this instance the software is

being designed specifically for an HCI experiment, and that the designer of the software is also the designer of the experiment, there is some flexibility in the subject positions that are open to the consultant. For example, on the one hand there are attempts to anticipate the effects of the laboratory environment on the subjects in both in terms of the stress they may experience; on the other, ways of preventing subjects 'doing things they shouldn't do' that might spoil the experiment are discussed (2.2: 22). Nevertheless there are a number of instances in which the consultant adopts the position of user's representative which are worthy of examination, and which can tell us something about some of the ways of speaking about users which feature in HCI discourse.

At the risk of oversimplification, this material can be read as suggesting that there are two modes of representation which are employed in the course of expounding the user's point of view. In one mode, the speaker talks in general terms, thereby implicitly presenting herself *as* a user and drawing on her own experience; in the other the user is explicitly invoked in terms of his or her likely reaction, were he or she to use the system. Here is an example of the first mode:

(2.2: 14-15)

Y: It's just that, I don't like to harp on about consistency but between some {of} them you actually have to perform that operation

F: Yeah

Y: of going into the catalogue, looking through and it kind of

F: Yes

Y: conjures up a mental model {that like you're in whereas} here you're in the help system and {all of a sudden} you're in the catalogue

The generalised 'you' is used here, and thereby Y's experience, recounted in cognitivist terms, is extended to anybody's experience. Elsewhere, the user is more explicitly defined and differentiated. For example:

(2.2: 22)

Y: You see if you were an {in}experienced user you may have a similar keyboard that if you press F1 does something

F: Yes

Y: and even though you're not

F: Yeah

Y: you may just do it automatically

The two modes are not entirely distinct, in the sense that it is possible to combine both within the same utterance; but they are quite distinct in their implicit claims. In the first, the representation of the user derives its legitimacy from the speaker's experience as a user: representation is based on identity. Whereas in the second, representation derives its legitimacy from a privileged understanding of the user; representation here is based on difference. Both modes can be found throughout HCI discourse though the latter is of more fundamental importance for the assertion of disciplinary expertise: for if representation is based on identity, there is no reason why software engineers cannot draw on their own experience as users and dispense with the services of HCI. The generality of the two modes is suggested by the following extracts from the standards committee meeting, in which the stakes are made quite clear. In the first extract, the chair asks whether anyone could envisage a situation where having become familiar with one variant of a symbol, they fail to recognize another:

(2.3: 15)

NB {} a little hypothesizing a scenario here, er could somebody put up the scenario where, some, where having learnt one of those, it, in another situation you wouldn't recognize, the user wouldn't recognize

It is noticeable that the speaker corrects himself here, amending 'you' - in this case a meeting of HCI professionals - to 'the user'. After a response has been given in which the speaker sets out his personal experience with a piece of software, the following exchange occurs:

(2.3: 15)

NB It's a case of moving from one system to another,

MP Yeah

NB it's a, and we're not typical users, we're trying to sort of, perhaps, if you think of office users who may or may, may even not be terribly experienced with these systems but have sort of played around with one of them, used one of these objects, moves to a system with a, with different obj, you know alternative objects

Here the contribution is ruled to be invalid since it is based on the speaker's own experience, and as such insufficiently representative of the point of view of 'office users'.

Whilst I have been at pains to emphasise that I am not interested in contrasting the texts of HCI with mundane speech and practice, these two settings may be of some relevance in this instance, for it would appear that the presentation of self as user is more widespread in informal spoken settings than in the literature[6]. Textual claims based on personal experience are open to charges on methodological grounds, especially given that the discipline's status is seen to be bound up with its credibility as science. For example, Long's (1989) comments on the limitations of what he sees as innovative work done in the seventies by Bornat and Thimbleby (1989) are based in part on the problems associated with situations in which there is an identity between user and researcher[7]: it is claimed that since the developers of the (prototype) system were also the people that were going to use it, their analysis of its strengths and weaknesses, whilst full of

---

[6] Within my own data this mode of speaking is omnipresent in informal HCI discourse (1.1, 2.2, 2.3).

[7] These comments were discussed in chapter three.

insights, has little potential for contributing to 'interaction development practice' in general since principles cannot be derived from it (Long, 1989: 20). Attempts to generalise from such personal experience would, presumably, be seen as too speculative to be credible.

In informal contexts however, it is often the case that the presentation of self as user has the advantage of providing grounds for comment in the absence of other users. In the case of the consultation for example, given that it is partly geared towards the anticipation of likely problems in the impending experiment, direct reference to the user is inevitably phrased in more provisional terms, and takes the form of representing the possible future problems of the hypothetical user: as such it is more speculative than generalised personal experience. To give two brief examples:

(2.2: 3)

Y: It's just that there may be where they'll select eighteen and they'll read, they {} umm they've read the question wrongly, and what they should have put down was twenty. Short term memory problem

(2.2: 16)

Y: I think that would be more

F: Yeah

Y: representative of that. Right we'll just go on with this one {} .. You see I don't think they'll read this once they've read it through once

There is of course nothing surprising in this observation; in the empirical absence of a constituency, representatives must speculate. However, it serves to support the more general point that HCI's representation of the user is best considered as a stance or position, within which there is considerable flexibility: for example, it may take the form of reporting on empirical instances of computer use, generalising one's own experience, or hypothesising a user's

probable reactions. Moreover, if the representation of the user is viewed as a position, that is, as a structural and defining feature of HCI discourse and practice, this reinforces the point that I argued in chapter three: that is, that the fundamental role played by claims to represent the user is a disciplinary one which helps to delimit and legitimate distinctive domain for HCI, particularly in terms of its relationship to the discipline of software engineering and the practice of system design. Let us now look more directly at the question of HCI discourse and its relation to design.

6.3. Design rationales

(2.2: 1-2)

Y: Are these, err, in a procedure which they follow {} that's the first thing they do?

F: Umm .. it's not necessarily set up like that, they're just the operations they can perform

Y: So it's quite arbitrary as to the way they're represented?

F: The order they're in

Y: But it's not is it, because open, close, previous, that's sort of the way you'd

F: That's the way I'd, I thought was a reasonable order to put them in but

Y: But what was that based on? (pause) .. I mean why have you got open catalogue rather than return to start at the top?

F: Well probably because open catalogue's what you do first so I thought it was

Y: That's much the same {in effect} the way you've ordered .. it's alright I'm just putting notes down so that I know

F: Yeah, yeah that's fine. I just hadn't even thought about why I'd put them in that order.

This is the first of numerous instances throughout the consultation in which the designer is asked about the reasoning behind particular design features: she is asked to supply a rationale for the design. As is apparent in this extract, and elsewhere, the consultant is making an inquiry about presumed design decisions, whereas the response throws into question the extent to which this aspect of design was the result of a decision as such. In this section I want to explore this recurrent feature of their conversation, and to suggest that it tells us something of general significance about the relationship of HCI practice to system design.

Sacks (1987) provides a useful way of approaching this issue. In discussing a brief telephone exchange between a suicidal woman and a counsellor, he begins as follows 'I do want to note that this question "tell me why you feel like committing suicide?" is in fact askable. That is, that it stands as a sensible and appropriate question to which there is expectably or reasonably an answer' (Sacks, 1987: 217). In a similar way, the consultant's questions about the design rationale can be treated as anthropologically strange; we can note that the question 'but what was that based on?' is an askable question, that the expectation that the designer supply reasons for the ordering of the items in question is a sensible one. It is worth noting in this respect that even though F is not able to supply an acceptable rationale for this aspect of design, her final utterance can be read as concurring that there were, or may have been, reasons for putting the items in the given order, but that she was unaware of what they were: on this reading, questions about design rationale are accepted as reasonable by the designer herself.

This interest in an underlying design rationale is a widespread feature of HCI discourse. Indeed, it constitutes the explicit focus and central feature of at least two methodological approaches to HCI practice. The Design Rationale framework is one such approach whose goal is to 'help designers reason about design and produce an output which can help others to understand why design is the way it is' (MacLean et al, 1990: 207)[8]. Here, a semi-formal notation is used to document the design process and represent design options as they arise. A different approach

---

[8] Software designers might legitimately object to the faintly patronising tone of this suggestion: presumably they are capable of reasoning about design without the help of an HCI methodology!

which also places design rationales at the centre of analysis is Claims Analysis (or Artifact Analysis), in which the the design of the artifact is assessed in terms of its implicit claims: for example, a given screen layout is seen as making, or embodying claims about its own design in terms of what is a reasonable way of structuring information[9]. Elsewhere, the rationale for the design being assessed may not be explicitly foregrounded, but it remains a pervasive feature of HCI discourse. The stance that HCI takes towards system design, which is related to the structural relationship of HCI and software engineering, is that of advocating that designers become more conscious of the design process, that they espouse design principles. The particular emphasis of course is that such principles should be derived from consideration of the needs of the user, but more generally HCI urges the importance of using explicit principles in the design process. For example, Edmonds concludes a review of his experiences in judging software for Design Council Awards, in which he stressed the need for design principles or a reference model, as follows: 'The experiences related in this paper have led me to go a step further and wonder whether it is reasonable to say that software has been *designed* at all if those principles, or the model, cannot be identified even by the authors of the product' (Edmonds, 1989: 52, original emphasis).

There is clearly a strongly normative element here: the assertion is not only that if a rationale cannot be given that the software cannot be said to be designed, but that it *should* be designed according to explicit principles. There is the further, and theoretically more problematic implication that if a rationale can be articulated by the designer, then the software is more likely to meet criteria for acceptable design. Both of these assumptions can be read in the recurrent exchanges on design rationale that take place during the consultation. With regard to the normative element for instance, it is noticeable that F's responses to questions about design

---

[9] For examples of this work see Kellogg (1990), and Bellamy and Carroll (1990). It is interesting to note that Carroll's brief outline of the theoretical basis for this approach at the Interact '90 conference describes its goal in very similar terms to MacLean et al: 'our goal is to make it more feasible to take the important objects of HCI practice seriously and thereby to empower designers' (Carroll, 1990a: 1057). The increasing attention paid to designers within HCI discourse is discussed further in the final chapter.

always tacitly admit the legitimacy of the inquiry. Generally this takes the form of attempting to give reasons, as we shall see below. On the one occasion no reason is volunteered:

(2.2: 11)

Y: Yeah {} .. Any reason why you've got that up there?

F: Um, I don't know really (laughs)

A plausible, if tentative reading of F's laugh is that it is self-directed: that is, it can be read as recognition of her inability to supply a reason in a situation where a reason is to be expected.

The requirement that the designer should be able to articulate a rationale for design, as expressed by Edmonds, is also implicit in the exchanges between consultant and designer. In assessing the system, Y's continual questioning is premised on the assumption that the articulation of a rationale could act as a justification for the design whose rationality was not immediately apparent. In other words, the designer's *intentions* are held to be relevant to the assessment of the rationality of the design[10]. There is a significant problem here with the implied relation of accounts to practice; as Mills (1940) argued, to take an account of the reasons for an action as an representation of the motivation for such an action is to misread language use as the unproblematic expression of inner thoughts and to neglect its social orientation. Beyond this, there is the implication that a rationality of design may lie behind the artifact, even if it is not apparent in the artifact itself[11].

---

[10] Carroll, by contrast, is careful to rule out the significance of designers' intentions for such an assessment, concentrating rather on the claims implicit in the artifact, as encountered by the user (2.4: 3).

[11] However, I may be legitimately accused of neglecting the social dimension of language myself here, since inquiries into the reasoning behind design decisions may be attributed to politeness and a reluctance to condemn without prior discussion. I contend however that the frequency of these exchanges suggests that they can not be plausibly reduced to this kind of explanation.

However, it is not the case that any reason can be offered in justification for a given design feature. There are tacit criteria of acceptability. This can be demonstrated by examining some of the exchanges in the consultation in which reasons are requested and offered. Let us return to part of the long extract that began this section, in which F was asked why a particular ordering had been used

(2.2: 1-2)

F: That's the way I'd, I thought was a reasonable order to put them in but

Y: But what was that based on? (pause) .. I mean why have you got open catalogue rather than return to start at the top?

F: Well probably because open catalogue's what you do first so I thought it was

Y: That's much the same (in effect) the way you've ordered .. it's alright I'm just putting notes down so that I know

F: Yeah, yeah that's fine. I just hadn't even thought about why I'd put them in that order.

In the first place, the ordering is justified in intuitive terms as 'a reasonable order to put them in'; however, this is not taken as a sufficient explanation, and Y asks what was the basis for this 'reasonable' ordering. She then reformulates the question more specifically with reference to the first item in the list. F's response is that it is 'probably' related to the order of the actions that the user would have to perform. (It should be noted in passing that the tentative nature of this utterance, and the admission in the final utterance that she 'hadn't even thought about why' lend support to the view that the articulation of a rationale should not be taken as evidence of the cognitive processes that informed the design). Shortly after this exchange, in which the designer has given two different reasons for the design feature in question, the consultant, whilst examining another area of the system, refers back to it in the following way: 'And these are just arbitrary as well are they?' (2.2: 2). Clearly the reasons that have been given are not deemed to be acceptable as a design rationale.

In other instances it is noticeable that the reasons that are given by F, in particular from the subject position of software designer, diverge radically from the kind of design rationale that is expected by HCI practitioners: that is, one based on explicit design principles. The following two extracts illustrate one kind of explanation that is offered:

(2.2: 4)

F: The problem was, the task got changed half way through, and its actually a lot of work to go through and change all these images so that if you choose another value it automatically {deselects}, and I wasn't sure whether, I thought probably this is OK to say you can {deselect} it by {clicking}

(2.2: 24)

Y: Why has the women's one got a {V} neck?

F: Well, originally, I had ones, I didn't divide them into women's and men's, I just had them all on one screen so I was trying to distinguish between men's and women's t-shirt, but then in the end I decided it was far too difficult, so I split them

In both instances, an explanation of the current state of the system is given in terms of a history of the design process itself. In the first extract, problems with the deselection function are attributed to changes in the specification at a late stage of implementation, and the difficulty of making wholesale revisions in the light of these changes. In the second, a redundant design feature is explained in terms of its place in a design layout that was subsequently abandoned. In both instances, design features are accounted for not as the consequence of explicit design decisions, but as the result of practical contingencies encountered in the course of design and implementation. My own experience of working in a software house suggests that, in practice, the impact of such contingencies upon the design process is considerable, and that temporal (and commercial) constraints act against attempts to minimise their effects. To this extent, F's

accounts can be taken as representing the mundane experience of software engineering, even if many would not be so open in admitting the role that contingency plays in their practice. HCI's insistence that design should be the result of informed and conscious decisions can be related to the external position that it occupies with respect to design practice; it is in a sense an idealist insistence in that it minimises the practical, material constraints under which software is designed and built[12]. I stress however that this observation should not be taken as a criticism: for the ideal itself is a laudable one. A final example illustrates well the divergence between the HCI ideal for design, and the software engineer's more practical orientation:

```
(2.2: 15)
Y:    Is there any reason why this one's small and this one's huge?
F:    Umm .. well that's supposed to represent a sort of normal
      screen, so I've just tried to get it the right shape and size
      whereas that's just a help screen and it can't be any bigger or
      it would be off the edge (laughs)
```

6.4. Description, prescription

The adaptive systems project can be located firmly within the discourse of HCI in a further respect: the extent to which such discourse is grounded in, and in many respects, derived from existing system designs. In this section I first examine the ways in which the project takes current state-of-the-art interface design styles as given, with particular reference to the consultation process. The terms of reference are then widened to briefly consider how far this essentially descriptive relationship to system design is seen as a problem by a discipline which aspires to the formulation of prescriptive design principles.

---

[12] In this repect, there are suggestive connections between the highly rationalist accounts of decision making that prevail within cognitive science and the prescription of explicit rationales for the design process described here.

It was noted earlier in the chapter that given the difficulty, or impossibility, of designing different interfaces of equivalent levels of difficulty, the emphasis was placed rather on making sure that each interface was typical of the class that it represented: these classes are identified as 'a command interface, a menu interface, a question and answer interface, a mouse and button interface, and an iconic interface' (Benyon et al, 1990: 573). The experiment design is therefore explicitly geared to finding correlations between cognitive characteristics and relative performance on existing approaches to design; and the evaluation of typicality was the main objective in the consultation process, with a view to amending any features that were thought to be unrepresentative. As the consultant's report puts it,

(2.2, Document 1: 4)

.. the focus of the evaluation reported here was to evaluate the different types of interface in terms of usability. The underlying objective was to determine whether the chosen interfaces were representative well-designed examples of the various types.

I suggest that this (perfectly reasonable) feature of the project tells us something further about the relationship of HCI to system design practice; for whilst HCI, as I have argued, has much to say about the way in which systems *should* be designed, it is also the case that any recommendations that it makes are formulated within a design space established by existing systems. Indeed, the existing history and state of system design is sometimes described in terms that seem to imply that they are the only possible types. The following extract from the consultation for instance, in which the taxonomy of interface types is further broken down into a taxonomy of possible menu interface types, illustrates well this formulation of the given quality of existing designs:

(2.2: 7)

Y: So what's this sort of based on the type of menu, you know there are several different types of menus that you can have

F: Yes

Y: pull down, pop up, pie and the type you get there which I don't know the correct technical term for

F: No I don't

Y: It's the one that

F: Well that's cas, that one was cascading, I mean the others are just pull down menus

Y: So you mix, you've combined pull down with {cascading}

F: Yes

Y: and that, you can take that to be sort of representative of menu, of a menu interface?

The first utterance 'there are several different types of menus that you can have' is suggestive in this respect, effectively ruling out, in normative terms, any approach to menu design not unambiguously located within the stated categories; the subsequent discussion turning on the further question of the typicality, and hence legitimacy, of combining certain recognized menu styles within the same interface.

There are two aspects of this approach that I want to note. The first is that it neglects the artifactual character both of currently existing interface designs and of classificatory systems. The availability of interfaces for classification is largely contingent on their success in the market place, whilst their classification is an essentially post-hoc activity, and not, as is often implied, a precondition of their design[13]. Secondly, it shows that there may be nothing else within HCI discourse on which to base the experiment: for example, that there are no principles for different approaches to design which could be embodied in innovative interfaces. It may be objected that the possibility of such implementation-free principles is chimerical; however it is fair to say that, as we shall see, it constitutes a genuine aspiration within HCI discourse.

---

[13] In some respects, the treatment of interface typologies within this project mirrors the realist approach to cognitive characteristics: in both cases, the constructive character of technologies of measurement and classification is underplayed.

There have of course been attempts to define design principles, but these have themselves been extrapolated from existing systems. To give an example of one of the most pre-eminent attempts in this direction, Shneiderman (1983) is generally credited with the formulation of the concept of 'direct manipulation interfaces', whose defining qualities were deduced from the study of a number of contemporary interface designs; subsequently, (Shneiderman, 1987) he went on to outline principles for interface design. But, for all their clarity, it is unclear how useful such principles would actually be for a designer who wished to do more than simply replicate the approaches to design upon which the principles were based.

The problem is how to move from description to prescription, and it is recognized as a significant problem within HCI by many of its practitioners, perhaps because the success and the status of the discipline within computing more generally may be bound up with it. It was, for example, a central issue in the Interact '90 panel session on 'New approaches to theory in HCI: how should we judge their acceptability?'. Monk's spoken introduction emphasised that all the participants shared the intention of moving beyond the empirically based guidelines that have constituted the core of HCI practice to date, and producing analytic approaches which can precede design. Of the positions outlined at this session, Carroll's 'artifact analysis' is of particular interest in that it entails a recognition of the innovative qualities of system design and the corresponding lack of any separate theory which can support further development; his ingenious and paradoxical response to this is to treat the artifacts themselves seriously as the embodiment of theory:

(2.4: 2)

JC: .. artifacts in situ are the er best theories we have. They codify, they embody what we know and they do it in a way that supports further development, and and er {} there isn't any other theory in HCI that does this, at least not yet.

This position is an appealing one in its implied shift from the exclusive emphasis on how to instruct designers to the idea that much can be learnt from design practice; however, in terms of the move beyond description and the avowed attempt to improve on existing designs, it is not clear how treating the artifact itself as theory constitutes a significant advance[14].

This discussion of the problem of moving beyond descriptive knowledge should not be construed as criticism in the sense of my suggesting that HCI *should* be able to produce prescriptive knowledge; for instance, there is plenty of scope for an HCI practice that empirically evaluates existing systems and their use. The aspiration to prescriptive knowledge for future design comes from within HCI discourse itself; it is increasingly concerned to occupy a more central role in the design process and to have a more significant impact on its future course than, arguably, has been the case to date. I will conclude this section with a passing remark of Carroll's from the panel session, which voices the discipline's concern that it may not have this impact; in the course of responding to the suggestion that a period of rapid and frequent innovation in artifact design was currently underway, he commented somewhat laconically:

(2.4: 3)

JC: We ought to get some tools that work during bursts of change and not just be victims to it so that later on we can spend that

---

[14] In fact, Monk's gloss on the common interest of the participants in producing predictive analytic approaches was a little misleading. Apart from Carroll, Harrison was concerned with the derivation of high level abstractions from existing systems which could be passed on to psychologists who would make necessary changes to those systems (Harrison, 1990), whilst Young (1990) discussed the development of genuinely predictive psychological knowledge via the construction of simulation models; but again this would entail prediction with respect to the effects of given designs. Long's overview of the three positions was explicitly critical of their failure to offer knowledge which had a 'prescriptive relationship with respect to the interaction' (2.2: 4).

quiescence trying to make sense out of what we had no effect on[15].

6.5. Conclusion

In this chapter, I have focused on empirical instances of HCI practice, in particular on certain aspects of one project, and attempted to relate it to some of the assumptions and structural features of the discipline as a whole. I take it that instances of HCI practice can be read as being shaped by the discourse of which they are a part, and consequently, I have not attempted to postulate ironic contrasts between the disciplines's texts and examples of mundane action. However, since the shift of emphasis to mundane practice sheds a different light on some themes, and suggests their further development, the relation of mundane practice to texts is not to be taken simply as one of exemplification. To do so would be to restrict the meaning of 'discourse' to textual materials; following Foucault, I have argued against such a restriction. Since practices are no less a component of a discourse than its texts, their study affords new insights into that discourse.

The examination of the adaptive interfaces project has focused attention on a number of the central features of HCI discourse that were discussed, or outlined, in chapter three. We have seen that the experiment is premised on certain notions of efficiency as an adequate measure of 'usability'; and we have further developed the discussion of the representation of the user, pointing to the cognitive conception that is utilised, and making some distinctions within the general stance of acting as the user's representative. Analysis of the interpretative practices

---

[15] Since the reference here to 'what we had no effect on' is clearly to be taken as, essentially, a joke, the reader may object to its citation to support a claim about the (serious) aspirations of the discipline. However, as Mulkay and Gilbert (1982) have shown, jokes can be illustrative of scientific discourse in that their very force as jokes derives from their resonance with existing aspects of scientific discourse and practice: in this instance, I would argue that its implicit self-denigration resonates with the disjunction between aspiration and current practice.

that are used to analyse the results suggests that they are framed by a set of explicitly realist assumptions. However, in this instance, the attention given to some of the temporal aspects of those interpretative practices provides some critical leverage; prior to arriving at a final interpretation which firmly locates significance within the phenomena of experimental performance, participants' talk can be read as undermining such realism via the acknowledgement of contingency and agency. This insight into the temporal dimensions of realism is an example of the way in which the study of mundane practice can complement the study of texts, highlighting as it does features of the discourse that are not so readily apparent in textual materials.

The central theme that has been developed here however is the question of HCI's relation to the practice of software design. This has surfaced in a number of ways. The advocacy of the user's position can be viewed in terms of the legitimation of the discipline's interest in design, in that it represents the user to the designer. Beyond that, HCI is seen as urging that the process of constructing artifacts should be informed by explicit design decisions, by explicit design rationales. Set against this however, we have noted that HCI theory and practice has an essentially descriptive relationship to design questions, despite its aspirations towards the formulation of prescriptive design principles. The issue of design and the role that HCI should play in it is central to the discipline; it is an issue of disciplinary assertion and alignment.

Although I noted the importance of temporal sequence in relation to the interpretation of experimental results, its significance was stated purely in analytic terms: that is, it directed attention to aspects of the phenomenon being studied. It remains the case however that the discussion of 'discourse' has tended towards the implication of a relatively static and determinate framework of objects, meanings and practices. To counter this false conception, I now turn to the examination of a further instance of HCI practice, which can be read as illustrating the process whereby an emergent discipline attempts to establish, stabilise and solidify its own distinctive discourse.

Chapter 7

Setting standards

This chapter continues the analysis of the discourse and practice of HCI. The empirical material is drawn from a single social interaction which takes place away from the computer: a panel meeting of the British working group on the development of standards for user interfaces. Whilst some material from this meeting has been used in conjunction with other data to support arguments made in the last chapter, here it forms the sole focus for a complementary but distinct discussion.

This event is of interest in a general sense because the collective discussion of a group of HCI professionals engaged in the definition of the optimal and standard form for icons provides a fruitful source of material for the analysis of the discourse of HCI. More specifically, the unifying thread that runs through many different aspects of the discussion that follows is the issue of representational adequacy. For the participants, for example, the issue arises both in relation to the adequacy of graphical representations on computer screens, and with reference to the problem of how far particular documents and spoken opinions are representative of national views, whilst for the analyst, the problem is similarly all pervasive: for example, the roles to which particular speakers and statements are assigned in the text, the problems involved in taking particular statements to be representative of particular positions, and the question of whether these individuals can or should be taken as representative of HCI practitioners in any general sense.

The specific analytic framework which is employed here derives in large measure from Woolgar's notion of 'the methodological horrors' (Woolgar, 1983). Elements of Garfinkel's ethnomethodology, in particular his discussion of the documentary method (Garfinkel, 1967: ch. 3), are developed by Woolgar into a full blown scepticism with regard to the problem of representational adequacy, which is stated in the following terms: 'what grounds provide the warrant for the relationship between the objects of study and statements made about those objects?' (Woolgar, 1983: 240). This problem is seen as irresolvable in principle, but managed in practice. Its discursive management therefore becomes a topic of considerable interest in the double sense that it involves the attempted reduction of the problem to the level of the merely technical, and that, given the impossibility of specifying an entity independently from its representation, participants' discourse can be seen as constitutive of its object. This perspective is therefore in line with the approach taken towards discourse in previous chapters, although it has a more specific interest in the ways in which speakers attempt to construct stable meanings for which there are no extra-discursive grounds.

Such a perspective entails a sceptical stance towards all discourse, whether academic or mundane. In this sense, to study the discourse of the panel is simply to point to a process of practical reasoning which occurs in all settings, in line with the contention made by some that ethnomethodology is indifferent to the substantive domain being studied (Hester, 1981; Sharrock, 1989). However, it should be stressed that the framework has been chosen here because it seemed a particularly apt one after an initial examination of the material. I therefore wish to argue that the applicability of a framework such as this can be differentiated in terms of its likely results in various domains. Whilst on one level I am, in line with previous chapters, simply advocating a general perspective on discursive phenomena which emphasizes their constructive character, and looking at the discourse of HCI from this perspective, I am also raising the possibility that a more specific emphasis on the problem of representation in relation to this material illuminates the sense in which the meaning of many fundamental terms and concepts in HCI are not firmly stabilized. The idea of the methodological horrors is used in order to give some analytical leverage on the phenomenon of establishing a distinctive discourse in an

emergent discipline, since it is contended that the work of this group can be characterized in these terms[1]. I am therefore claiming that such an approach can be used in order to learn something substantive about this field[2].

7.1. Background

The primary focus of the discussion that follows is a meeting that took place in October 1990 of the British working group IST18/-/9U. This group is part of the British Standards Institution and its work involves, notably, the formulation of a national position on, and contribution to, the development of international standards for user interfaces in the international committee JTC1/SC18/WG9U. Its two main current functions are the definition of standards i) for 'icons and other graphical symbols' on computer screens, and ii) for the definition of 'a basic set of objects and actions common in office systems' (Stewart, 1990: xxxii). Of these two functions, only the first is studied here. One of the participants furnished me with an audio tape recording of the meeting, from which a transcript has been made. In addition, documents which are referred to in the meeting, together with some which relate to previous meetings and some which are in part the outcome of discussions during the meeting, have been consulted.

The purpose of a defined icon standard cannot be unproblematically stated at the outset, since this, and even more fundamental issues such as how an icon itself should be defined, is a matter for debate amongst the participants themselves. The guiding assumption however is that standard definitions in this area are intended to in some way enhance 'usability'. A useful provisional statement of this can be found in the report of the last international meeting which states that the different national bodies 'all strongly support the principle of a standard, with

---

[1] In a sense, I am also using ethnomethodology's stress on the active and temporal accomplishment of order and meaning by participants in order to supplement the largely synchronic approach to the analysis of disciplines suggested by Foucault's work.

[2] However, the question of to what extent the phenomena described in this chapter could be generalized to other emergent disciplines must remain an open one.

the objective of producing more consistent and easier to use interfaces, but disagree on how this is to be achieved' (2.3: Document 96: 1). This statement nicely sets the tone for the range and depth of issues that are raised during the meeting, since the disagreement on suitable means throws into question whether a standard is the best way to achieve such ends; the commitment in principle may turn out to be nothing more than an article of faith.

Both industry and the academic world are represented on this panel, the former predominating. On this occasion, there are eight people present, and the main emphasis is on the formulation of a distinctive UK position on the form a standard should take, in the light of fundamental disagreements between, in particular, the US, German and Japanese delegates at the last international meeting.

7.2. Representatives

The process of working towards the formulation of a distinctive UK position on standards involves the discussion and evaluation of different statements, in both written and spoken form. A crucial issue is the question of which group or constituency a speaker purports to represent, and the closely related one of the extent to which statements can be taken as representative of that constituency. This is the first aspect of representation that I want to examine, but it should be noted that it closely mirrors and interconnects with the substantive topic under discussion by the panel, namely the optimal form of a standard for visual representations. As Latour (1990) has argued, the representation of people and their opinions, and the representation of things can be fruitfully viewed as aspects of the same phenomenon. Both are characterized by considerable interpretative flexibility.

Contenders for the constituencies being represented include: users, the HCI community, designers, computer manufacturers, nations, personal opinion, and the voice of common sense. The difficulty for any analysis is not simply that speakers at different times claim, explicitly or implicitly, to represent different groups; rather, participants actively use this indeterminacy as a resource for arguing for different positions. The analysis here then focuses on this variability rather than

trying to resolve it; indeed the production, management and utilization of variability is part of the topic of interest (see Gilbert and Mulkay, 1984)[3].

The prevalence of formulations within HCI discourse which purport to represent the user, in the double sense of speaking on behalf of and constructing a model of him or her, has already been noted: this meeting provides a number of instances of these, as discussed in previous chapters. A closely related position, whilst not mentioning the user, asserts that an important role of such advocacy is to combat the indifference or hostility of software engineers and other computing professionals. Here the task of making representations on behalf of the user is elided with, and provides the legitimization for the assertion of professional self interest in relation to other professions[4]. For example, in the early stages of the meeting, prior to the discussion of icons, the following remark is made in relation to the drafting of a standard for the 'human-readable' form of electronic mail addresses:

(2.3: 3)

NB: I was the only person from ISO in the group working on it, and I was also I think the only person in the group who had any human factors experience so if I'm not able to attend, um the thing will get treated rather differently and might even be treated with a, we're not having any of this type of attitude.

Other panel members are therefore urged to take a role in this group to ensure that the HCI position is adequately represented, and thereby to counter indifference and hostility. It should be stressed however that this hostility is of course itself a representation of the attitude of a vaguely defined constituency, and one which serves the rhetorical function of reinforcing the

---

[3] It should be noted however that this focus on variability does not exempt the text itself from the process of ascription which is its topic.

[4] This is consistent with Foucault's view of the development of disciplines as the definition of exclusive domains of knowledge and expertise (Foucault, 1977).

necessity of getting people with 'human factors experience' involved. The rhetorical effect derives firstly from the contrast of a dismissive attitude with the, implicitly, more understanding approach that would be adopted by an HCI practitioner, and secondly from the formulation of an embattled position: if HCI is not represented, ground will be lost.

In this instance then, the HCI community is explicitly invoked as the relevant constituency in a relatively unambiguous manner, although even here it is apparent that such ascription achieves certain rhetorical effects: that is, it works to affirm the contribution that representatives of that constituency would make if they were able to attend future meetings of the group in question. If we examine the invocation of other constituencies in relation to actual, rather than future hypothetical statements, this process is even more marked. In particular, ascribing different constituencies to different statements is one way of supporting or undermining their legitimacy. Again, it should be stressed that this process of ascription is an important aspect of the construction of realities which are seen to underlie different statements; claiming that these statements are or are not representative of different constituencies and positions gives coherence to, and discriminates between, a wealth of conflicting opinions on the topic of icon standardization.

The representation of national views is of particular interest in this respect. At the beginning of the meeting, there is extensive discussion of the different positions expressed by Germany, USA and Japan at the last international meeting, and in associated documents, on the precise form that an icon standard should take. The United States is said to favour a liberal position which gives maximum freedom to the designer, whilst the Germans are said to tend towards a more prescriptive approach. It is worth noting the way in which the chairman conveys the latter position:

(2.3: 4)

NB: The Germans, with some support from the Japanese, er have all along, and with delegates who are essentially er designers [..]

have all along imagined that what they should be doing is producing symbols rather like equipment symbols

That the German position is misguided is the clear implication here; furthermore, this can be attributed, at least in part, to the professional orientations of the individuals who comprise the German delegation. They are designers, not 'human factors' specialists. We see here both the process of self-assertion that has already been noted, and an interplay between individuals, professions and nations. The interplay between individual and national positions is a much used resource. For example, there is agreement that whilst the Japanese position is in some respects similar to the German one, it is difficult to ascertain precisely:

(2.3: 4)

TC: {Well} the Japanese are claiming to have something of a different view but {}

NB: No I couldn't either

TC: see anything special about it

This is explained in terms of the difficulty of knowing how far individual delegates are accurately representing the views of their national bodies to the international meeting:

(2.3: 5)

MP: No er, difficult to work out the definitive Japanese position, I mean it varies very much with who speaks, I'm sure they have a position but they don't put it over.

Here the speaker attributes the imprecision of the Japanese position to the problem of distinguishing individual's representations from the position itself; an interesting feature, however, is the certainty expressed that a coherent national position underlies the different

accounts that are given of it[5]. This kind of certainty is a feature of participants' practical reasoning throughout the meeting, and serves, in Woolgar's terms, as a way of dealing with the methodological horrors: that is, the assumption of a stable signified beneath diverse and conflicting accounts deflects and defers facing the possibility that, in this case, there is no essential Japanese position that can be separated from accounts that are given of it, or elsewhere for example, that there is no stable entity called an icon that exists beneath and apart from its divergent definitions. The most striking statement, viewed from this perspective, is the following, where a particularly convoluted hypothesis is constructed in order to resolve the imprecision of one individual's account with the assumed coherence of the position it claims to represent:

(2.3: 4)

NB: They didn't state their view very clearly in that. My understanding of their view was that er, I mean the trouble is that the most eloquent and um experienced Japanese delegate I think holds the more liberal view than he believes his national body holds, so he may have deliberately er drafted a fuzzy statement which was consistent with his national body but didn't actually (laughter) {} in the hope that the national body would be persuaded to be more flexible

A further dimension to the complexity of representation is provided by the presence on the panel of representatives from companies with an interest in the drafting of a standard and the effects it may have on them.

---

[5] The procedural means by which some 'UK' positions are arrived at during this meeting suggests that this might be a misleading assumption (see below 'Managing the horrors').

(2.3: 12)

TB: Who, who are the people that are going to implement the standards after {us}?

{JE}: Big, the big suppliers

NB: Well I mean clearly the big suppliers, and as long as we're in desktop icons it's only the big suppliers, if we get down to drawing kit icons then it's much wider applicability.

TB: So we could be drawing up standards for IBM and Mac?

NB: Oh absolutely, that's why they're represented here.

Again, this provides participants with a resource for making evaluative judgements. Whilst it is never contested that it is reasonable that major companies should be represented, particular positions can be put in a negative light if they are seen to be too closely derived from commercial interests. The following extract occurs during a section of the discussion in which the garbage can is used as an example of an icon that would be standardized. Here, the Apple delegate (not present at this meeting) is represented as acting uncompromisingly and unambiguously on behalf of corporate interests:

(2.3: 13)

{RH}: The garbage can's an established convention with Apple,

NB: Absolutely right

{RH}: I understand they're very fiercely protecting {} their interface

NB: Well, we're led to believe that they would fiercely protect their particular representation of a garbage can, but we're probably ok on other garbage cans drawn slightly differently

{}: have the handles in a different position {} the top

NB: Yes {} with or without, I mean, Pauline was very protective of the bars down the middle, we haven't yet managed to persuade her we're allowed to do a garbage can.

TC: It is actually an Apple person drafting the standard {}?

NB: That, that was an Apple garbage can you see, so er

{}: They'll lead us into a trap {one day} (laughter)

The discussion here begins with an acknowledgement of Apple's legitimate interest in the development of a standard which has implications for some of the more distinctive features of its own designs. However, TC (an IBM employee) casts doubt on the legitimacy of this position with his ironic remark to the effect that one company is calling the shots in an unfair manner; whilst the final utterance suggests, perhaps not seriously, that this self-interest may have undesirable consequences. Of course, representatives have to be alert to the charge that any opposition expressed is also simply self interest. This is achieved below by satirizing such self interest:

(2.3: 16)

TC: I think you should define it so that Macintosh has to change theirs. (laughter)

In these extracts, the presence of commercial interests is relatively explicit, and the dispute hinges on judgements as to whether a mark has been overstepped. Elsewhere, commercial interest is something that can be alleged, and construed as being in opposition to principle[6]. That participants are aware of the damaging implications of this is most clearly shown in the following:

(2.3: 6)

NB: Well, that is another, another issue which we might wish to address which is what the current scope of the standard should

---

[6] We shall see below that this separation of principle from other opposed positions is never given. The difficulty of separating principle from pragmatic commercial considerations derives in part from the conflict between HCI's aspiration to formulate principles for good design, and its parasitic relationship to the existing designs of major companies.

be and what the future scope of the standard should be. That has caused some confusion, but as currently documented, we should look at what the words say, it applies to object icons but Simon is also taking an IBM view {} says that all icons

TC: I don't think that {} (laughter). It's common sense! (laughter)

It is clear then that the ascription of constituencies in relation to particular statements is an important part of participants' interpretative work. In the face of fundamental uncertainties as to which constituencies are being represented in statements, and how far statements can be taken as representative of such constituencies, panel members attribute constituency as a practical way of resolving uncertainty, constructing the positions that statements are said to represent, and using these constructions to make evaluative judgements. Moreover, the process that is discernible here supports my claim that a discipline such as HCI can best be understood as a discourse, rather than, for example, an identifiable group of practitioners: for membership of the discipline is just one of the available constituencies that can be invoked.

7.3. What is an icon?

Given the prevalence of graphical user interfaces on computer systems, and the consequent perceived need for an international standard for icons, a degree of consensus on key components and terminology might be expected. For example, one would expect that agreement on the desirability of standardizing icons presupposes some form of agreement on the meaning of the term icon. In one sense this is the case, since participants' ability to discuss the question of what an icon is necessarily demonstrates some shared understanding of an icon as a unitary entity. However, this turns out to be a practical achievement in the face of their inability to define what an icon is; and indeed, the further the discussion on this topic proceeds, the more definitional difficulties are encountered. It may be more appropriate to view these difficulties as being generated by the very activity of trying to fully explicate and define icons (cf Garfinkel 1967: ch. 2).

An initial sense of the nature of the problem can be gleaned from a document which was circulated to participants prior to the meeting. This is a revised draft of a proposal for an ISO standard for graphical symbols for use on screens, to which a letter from the writer to the chair is attached. This letter describes some of the changes made from the last draft, and the reasons for them. It makes it clear that the difficulty in reaching an agreement on the definition of an icon is not confined to the meeting under scrutiny here, but has been a prevalent feature of previous meetings and documentation. The main problem is represented in this document as deriving from the graphical and interactional character of icons; the writer is concerned that an inappropriate definition has been arrived at:

> (2.3, Document 100: 1)
> Abstracting across the invariance then, the term 'icon' refers to the symbolic component of the interface object, with the consequence that icon=symbol. I thought we had gone to great pains to ensure that this should not be understood to be our position.

The preferred position for this writer is that icons are 'subclasses of interaction device' (ibid: 1), and that such a conception 'cuts out some of the fuzziness and equivocation that we've had in the past, about not knowing what an icon is; it does go back to the original conception of an icon in user interfaces and is quite clear that an icon is not a symbol, although it does have a symbol or graphic associated with it' (ibid: 1). The cause of this 'fuzziness and equivocation' then is the fact that an icon can be perceived both as a graphical element, and as an interactive one; the mistake is to take the former aspect, appearance, as the defining characteristic, since this neglects the second, function. This formulation however fails to put an end to interpretative difficulties, as one might expect given the simultaneous recognition that an icon has a graphic 'associated with it'; for it is not clear whether this association is itself a defining characteristic. The definition of an icon given in this draft begins 'A graphical element of the user interface to a computer-based application .. ' (ibid: 6). However the next draft, tabled at the meeting has changed this to 'An element of a graphical user interface to a computer-based application .. ' (2.3, Document 106: 5).

The problem posed by this dual nature of icons is well summed up, at the end of the discussion, by the chair:

(2.3: 27)

NB: Yeah but we've got a big problem, if you say that if the thing you, the thing can behave functionally like an icon, and even if it's not a picture you'll probably call it an icon. On the other hand you've got things which don't behave like icons but they look like icons

This statement is of especial significance, for not only does it spell out the dilemma faced by those attempting to define an icon; it illustrates perfectly the process of practical reasoning described above, for the speaker deploys a sense of what icons actually are, whilst pointing out the difficulties raised by alternative definitional criteria. In effect, he appeals to a sense of what everyone knows an icon to be with the phrases 'like an icon' and 'like icons', as part of an argument about the problems of defining icons in a certain way: and yet it is precisely the difficulty of specifying what an icon is, which has been the topic and difficulty of the preceding discussion.

This choice between appearance and behaviour as candidate definitional criteria is only part of the difficulties negotiated by the participants, and is itself more complex than has been suggested in the extracts quoted, since there is also the question of precisely what the graphical form of the icon should be taken to represent. For example there is an extensive exchange on the definition of a standard for a waste bin icon (2.3: 10-19), and the question of whether two forms, waste bin and waste paper basket, might be acceptable. The problem that emerges is that there is considerable uncertainty as to whether permitting this would amount to permitting legitimate variations of one icon, or whether this would rather amount to standardizing two different icons. As the chair states, 'Now the question is are they different objects or the same objects for the purposes of this discussion?' (2.3: 12). The two different views of visual representation that are

utilized here are, firstly, the perhaps more intuitive one that the visual appearance represents the function that the icon performs; and secondly, that it represents an object which in turn stands for a particular function. The former tends to be implicit in much of the discussion, whilst the latter is more explicitly stated:

(2.3: 18)

TC: {Really the idea}, what we're actually representing is a three dimensional object.

In introducing a mediating object between appearance and function, this position adopts a rather literal or realistic theory of signification which is, as we shall see below, in keeping with the general tenor of much of the discussion. It appears to offer an adequate resolution of the specific problem with waste bins and waste baskets; but it also introduces other uncertainties to the general problem of defining icons, when considered in relation to the further issue of how inclusive or exclusive the category of icon should be. At one extreme is the general and inclusive definition, applauded by many participants, that an icon is a 'non-technical representation of something' (2.3: 26). However, it is immediately pointed out that this would admit entities, such as boxes with text in them, which would not usually be thought of as icons. The other extreme is that icon should exclude certain classes of entity such as 'tools' or 'pointers/controls'. One point of difficulty here is that these two classes are contrasted with the category 'object'. Object then has a potentially ambiguous status as that which is represented by any screen image, and as a certain category of entity which is distinct from others, and would contain such entities as the waste bin. The latter introduces yet another problem. One suggestion is that pointers should be excluded from the category icon, which would include tools and objects, but it is pointed out that:

(2.3: 25)

NB: You've got a problem cause a pointer can also be one of these objects can't it?

MP: Yes, yeah same image.

Two separate and exclusive categories could therefore share the same image if this categorization is accepted, a situation that appears to be strongly counter intuitive to many.

It is evident therefore that the initial work of defining an icon, and therefore of determining the scope of the group's work, is fraught with conceptual difficulties. Whilst there is a general and intuitive sense of what icons are, a sense frequently brought into play by participants, attempts to formulate more specific categorizations and definitional criteria come up against the imprecision of key concepts; further, particular categorizations introduce further problems of their own.

7.4. Visual representations

The work of defining standards for icons and other graphical symbols entails, at least in part, the working out of an optimal form for a given icon or symbol. In discussions, participants therefore utilize theories of visual representation, albeit implicitly, in asserting a preference for one visual form over another, since to make such an assertion is usually to make a claim about the representational adequacy of that form with reference to some criteria: these criteria are premised on theories of how visual representations signify. In this section, I want to look at such implicit theorizing, again from the standpoint of a concern with the fundamental uncertainties involved in such work and an interest in the ways in which they are managed and negotiated in practice. The aim is not therefore to argue with a theory, but to make explicit some of the assumptions which appear to underlie statements: of course, in attempting the latter, this text participates in, and exemplifies the practical management of fundamental uncertainty by discursive construction which is its central topic.

Central to much of the discussion of the representational adequacy of icons is the concept of the 'desktop metaphor'. The first systematic use of icons was in the Xerox Star user interface (Smith et al, 1982), which attempted to present a coherent conceptual image of the different functions of the system by means of graphically representing them in terms of elements that would be found

in an office. The use of the term 'metaphor' to describe such a visual design has subsequently become widespread in HCI, whilst the desktop metaphor has become widely used. The rationale for this approach is stated by Carroll et al as follows: 'Instead of reducing the complexity of an interface, this approach seeks to increase the initial familiarity of actions, procedures and concepts by making them similar to actions, procedures and concepts that are already known' (Carroll et al, 1988: 67). This is an uncontroversial explanation of an unquestionably successful approach to design, but it is interesting to note that in accounting for its intelligibility the emphasis falls on the correspondence of elements on the computer screen to known elements from a different domain, for instance the user's office. This is a not uncommon position in HCI as a whole, and is certainly prevalent in this committee. The aspect that is played down is the intelligibility that derives from the internal relations within the conceptual unity of the metaphor[7].

Much of the discussion of representational adequacy that takes place at this meeting is located firmly within this paradigm. Whilst there is recognition of the significance of metaphors for definitional work, for instance in terms of determining the possible scope of the standard (see next section), specific icons are assessed in comparative isolation from other elements of the screen design, and primarily in terms of their fidelity to 'real world' objects. An explicit statement of this can be found in two successive drafts of a key document:

> (2.3, Document 106: 3)
> This standard is concerned with the relation between individual icons and their referents, however, and not with the metaphorical scheme within which they are deployed.

Before examining the consequences of this approach for the discussion of representational adequacy, it should be noted that it is opposed to the theoretical framework which informs this

---

[7] Robinson makes a related point in suggesting 'formality' as the salient feature of such systems (Robinson, 1990: 48).

thesis. The latter is located within a general movement in some of the social sciences, in literary theory and in continental philosophy, which takes a sceptical stance towards any model which explains meaning and intelligibility, whether of language or signs more generally, in terms of adequate representation of a referent. One key source for this is de Saussure (1978) whose argument that the meaning of words and signs are derived from their relation to other elements in the signifying system is explicitly opposed to this statement; this provided a central foundation for structuralism, semiotics and deconstruction. Ethnomethodology also takes a sceptical position on such a realist theory of meaning (Garfinkel, 1967; Woolgar, 1983). The statement therefore has a distinct theoretical orientation; it may be, whilst not reducible to it, that it is related to the cognitive paradigm that dominates HCI, in that comprehending meaning is seen as a process in which an individual infers the correct referent from a signifier.

As a consequence of this realist approach to signification, representational adequacy is assessed in terms of resemblance to 'real world' objects, and intelligibility therefore turns on how familiar such objects are to users[8]. For example, one of the arguments for flexibility in icon specification, and for the possibility of allowing for customization, turns on the existence of different types of folders in different countries; the most 'natural representation' for each user group, it is argued, would be of 'the physical folder we all use on our desk or in our filing cabinets or whatever' (2.3: 10). The adherence to this theory is even more strongly demonstrated with reference to the acceptability of different symbols for the waste function; here it is not simply argued that similarity to known objects enhances intelligibility, but that the absence of such objects in the office casts doubt on their efficacy as symbols:

---

[8] Baudrillard's theory of simulations and *simulacra* (Kellner, 1989) provides an alternative view of signification which, although overblown in its universalistic claims, may be an apt one for the domain of computer images. Increasingly, the mediation of experience by images (for instance in advertising) is transformed into the absence of 'the real', and the all pervasive presence of *simulacra* in their place. Thus, the desktop metaphor's depiction of folders, documents, waste bins and filing cabinets would stand as a simulation of a pre-electronic office at the same time as the computerization of office work began to render such objects obsolete.

(2.3: 12-13)

TC: Well the thing, the lid is {} there, I've never had one with a lid at work

[..]

MP: You don't have one of those galvanized cans in your office (laughter)

The criterion suggested here, and elsewhere, implies that the elements of the metaphor should be strictly homologous to the domain to which they refer. One peculiarity of the desktop or office metaphor, which reinforces such a position, is that it refers not to a wholly distinct domain but to the very environment in which the computer is used[9]. One speaker, immediately following on from the last extract, challenges this narrow approach to metaphor:

(2.3: 13)

{RH}: Some people do (laughter) .. in some ways its a very appropriate metaphor to use {} There, there are metaphors which you may want to use on office systems which {actually} don't

MP: {}

{RH}: use an office.

Nevertheless, the approach that prevails throughout is undoubtedly the realist one. It is assumed that the intelligibility of an icon is directly related to its resemblance to the relevant object in the 'real world' of the user's immediate environment. Photographic and holographic images, whose increasing use on future computer systems is seen as probable, are therefore the 'closest to the referent object, the object you're actually referring to' (2.3: 18). The assumption of

---

[9] This is one reason why discussions of (visual) metaphors in HCI seem quite separate from discussions of metaphor in other fields, notably philosophy, where emphasis is placed on the creative, novel and unexpected qualities of metaphorical descriptions (see for example Davidson, 1984).

many participants is that a photographic image, since it more directly represents the object, is therefore the most easily recognizable. This seems to underlie the view that the increasing use of such images is likely. Law and Lynch's study of the representational styles of different birdwatching manuals throws this into question, arguing that a more caricatural style which is able to emphasize key elements and suppress less relevant detail is more effective and recognizable than photographic images (Law and Lynch, 1988)[10]. The Macintosh trashcan may therefore be a more useful representation than a photograph for this reason; moreover, it's use of explicitly non-realistic devices, such as bulging when things are thrown in, may further enhance its intelligibility.

Leaving such doubts aside, the possibility of 'direct' photographic representations puts into focus some of the difficulties with the 'realist' approach. If photographic images can represent all the details of a referent object, a detailed standard could be specified which ensured more consistency of images across different systems 'than you get by merely specifying a minimum number of elements' (2.3: 18). On the other hand, such detailed representations afford the possibility of accurate but non-standard representations as the following extract shows:

(2.3: 18)

NB: I was going to say you could have a photograph of the very trashcan you have outside your door,

MP: Mm

NB: and of course it doesn't help when you ship the system off to Africa, but er

YR But the other argument is that there are a number of different trashcans that you can buy,

NB: Mm

YR which one do you take as your exemplar

---

[10] They reach this conclusion after analysing the situated use of manuals in the field.

At least two important issues are raised in this sequence. The initial utterance pushes the idea of reference to familiar objects to an extreme in which an icon could explicitly represent an analogous object in each user's environment. It might be argued that this is the logical extension of approach to signification that has been discussed here. The first problem, which is addressed by speaker YR, derives from the centrality of the notion of an object to this approach: since there are many variants of such 'real world' objects, what would be the grounds for choosing between them? Here again, the idea of a mediating represented object that stands for a given function, rather than resolving some problems of specifying and standardizing icons, instead introduces further indeterminacy since it brings into play a whole range of possible referents. Furthermore, as another speaker remarks in relation to the recognition of objects - in particular, filing cabinets - 'sometimes I have problems working out {}, even in the real life context' (2.3: 19). The problem of intelligibility is merely deferred by an approach which considers the meaning of an icon purely in terms of its reference to 'real life', since the same problems of uncertainty apply equally there.

The second area of difficulty derives more specifically from the requirement that such objects should be familiar elements of the user's environment. This poses a fundamental problem for the viability of the whole project of standardization; this is recognized, in terms of international standards, in the same speaker's comment that 'it doesn't help when you ship the system off to Africa'. The problem is one of representational adequacy across different cultures, and it is one that figures prominently throughout the meeting. If intelligibility is so closely tied to the features and objects of the different environments in which systems are used, then a uniform international standard may be difficult to achieve, and possibly even undesirable. This difficulty is recognized by the group as compromising the ideal of maximum intelligibility via the familiarity of the referent:

(2.3: 9)

NB: You may, it should be easy to learn but you may have to learn it. If you accept that, then the fact that the waste bin doesn't mean much in your African culture may be something you have to

live with, just like the American folder doesn't mean much in a European culture.

Moreover, there is the further difficulty of actually finding out what would be valid and intelligible images in different cultures given that 'international representation on the committee is very limited'; the solution proposed in this regard is to 'rely on the big, the multinationals who report back' (2.3: 12). In this respect, two further problems of representational adequacy in relation to cultural difference are introduced: are different cultures adequately represented at meetings, that is do they have reliable representatives, and are the representations of those cultures that are constructed at meetings adequate[11]? The point I wish to emphasize here however is that the perceived difficulty presented by cultural difference is thoroughly intertwined with the theory of signification that is in use.

7.5. Standards

One area of uncertainty that has already been alluded to is the question of the precise purpose and scope of the proposed standard for icons. As was noted at the beginning of the chapter, the different international bodies are said to agree on the desirability of a standard in order to improve usability 'but disagree on how this is to be achieved' (2.3, Document 96: 1); uncertainty here is presented as being simply on the level of devising suitable means. However we have seen that some of the views put forward at this meeting introduce the possibility of more

---

[11] A point of some interest is the way in which discussions of cultural difference often revert to 'primitive' metaphorical representations, even though the domain in question is the modern computerized office. For instance, in the extract just quoted, the example given by the chair is that 'the waste bin doesn't mean much in your African culture'. This is satirized by a subsequent speaker: 'Where do the Africans put their waste then?' (2.3: 9). Or again, in the context of a discussion about testing for conformance by means of seeing whether users can recognize a given icon, 'You'd take one {} and none of them would recognize it, mm, you'd get a little tribe in the depths of Borneo {}' (2.3: 19).

fundamental doubts about the standard. In this section, some of these doubts are briefly examined in a little more detail.

The rationale for having a standard for icons and graphical symbols, beyond the broad aim of improving usability, remains unstated throughout this meeting and in the draft proposal documents which are discussed. In the letter to the chair that prefaces a revised draft for one part of the standard (Document 100: Graphical Symbols for Use on Screens), the writer comments on an earlier draft:

> (2.3, Document 100: 1)
> One shortcoming I found was that there was still no definition of what an icon is, or for that matter what is the purpose of the standard, or the purpose of icons. Accordingly, I have much elaborated the definition of icons in the Definition section

The point of interest here is that the writer's response to these perceived inadequacies is merely to further elaborate the definition of icons; there is no statement of the purpose of the standard in this or the subsequent draft. It is against this background that some of the issues that have already been discussed can be clearly seen as potentially disruptive. For instance, the existence of cultural differences, especially in conjunction with the predominance of what I have called a realist theory of signification, suggests that an international standard may compromise usability as far as some user groups are concerned. If the logic of this approach is followed to its extreme position, usability might be better served by a state of complete deregulation in which each user could have a 'photograph of the very trashcan you have outside your door' (2.3: 18).

A further problem is implied by the participants' discussion of the closely related problem of 'real world variation'. One speaker noted that given the existence of many different waste paper baskets, there was a problem in deciding which should serve as the exemplar for the standard (2.3: 18). A similar point is raised in relation to the issue of whether waste bins and waste baskets should both be permissible (2.3: 12). But in addition to the problem of what should serve

as exemplar, a further difficulty can be inferred: since waste baskets themselves are not standardized, why should representations of them on computer screens have to be? Of course, this is not to say that reasonable arguments could not be put forward for such standardization; for example, one speaker's comment, already noted, that he has difficulty recognizing filing cabinets, might suggest that one should try to improve the intelligibility of the computerized domain in relation to the incoherence of 'real world' objects. The point however is that no such argument is articulated. Part of the problem here is that two aspects of standardization are continually elided throughout discussions: what kind of symbol should represent a given function, and what the precise form of such a symbol should be.

The other source of considerable difficulty is uncertainty about the scope of the standard:

(2.3: 6)

NB: another issue which we might wish to address which is what the current scope of the standard should be and what the future scope of the standard should be. That has caused some confusion

The speaker here goes on to elaborate in terms of whether the standard should apply simply to object icons or should include other symbols. The issue in this respect is therefore very closely related to the question of 'what is an icon?' since clearly the icon standard is dependent upon definitions of what is to count as an icon; accordingly, this aspect of the scope of application will not be discussed further here. Instead, another aspect of this issue will be examined: is the 'desktop metaphor' the substantive topic of discussion, and the intended area of application of the standard, or merely an example of a wider class of designs?

Ambiguity on the question of the status of the desktop metaphor can be related to the relationship that HCI has to existing system designs: that is, it attempts to derive from descriptions of systems underlying concepts which can be specified and can form the basis for prescriptions for future designs. As was mentioned earlier, the first extensive use of icons was within the desktop metaphor, and this metaphor has subsequently become widely used. Icons are

therefore closely tied to their original use within this metaphor in conceptual terms, notwithstanding the enterprise of specifying standards for icons *per se*. It has already been noted that the most recent draft proposal for part of the standard comments that it is not concerned with the metaphorical scheme within which icons are used (2.3, Document 106: 3). On the same page, a more equivocal formulation can be read:

(2.3, Document 106: 3)
Typically, but not necessarily, the graphical user interface will employ a variant of the desktop metaphor to illustrate application concepts.

In the course of the meeting there are some brief attempts to talk about icons within a wider framework, but the discussion continually reverts to the desktop metaphor in the apparent absence of obvious alternative examples. Consequently, a process can be construed in which the desktop metaphor moves, by default, from being the example used for the purposes of discussion to the substantive topic. The following extract initially sets the tone for the discussion:

(2.3: 6-7)
NB: we're dealing at the moment only with objects presented to users of an application for inspection and information, representing a corresponding physical object which serves the same function. In other words, we're dealing loosely with the desktop metaphor
JE: Mm
NB: but clearly not necessarily restricted to, it may, they may also be applicable to other metaphors which er, are there for the same purpose as the desktop metaphor is used for, I think that's the way I would {interpret} that ..
YR: What other types of metaphors are around?

NB: Well that is one of the problems you see, because do we write this and say it's only desktop metaphor because if somebody comes up with another metaphor, it's ..

The chair here states that 'at the moment' they are 'loosely' dealing with the desktop metaphor, but also that the standard 'may' be applicable to other metaphors. On being asked about which other metaphors might be relevant, he begins to discuss the problem of how far the standard should attempt to be applicable to future designs. Given that the ensuing discussion is concerned with such details as what the correct symbol for a waste receptacle should be, and whether or not it should have a lid, it is difficult to see how the standard could possibly anticipate and deal with future designs and metaphors. However, the question that is avoided here and elsewhere is that of currently existing systems that use different metaphors. The only real attempt to move beyond the desktop metaphor as example is a brief discussion of a hypothetical icon, a black hole (2.3: 9-10), but this is dismissed by one speaker on the grounds that it 'isn't part of the desktop metaphor' (2.3: 10). Accordingly a 'more plausible example' is chosen: a waste bin (2.3: 10). In fact, the black hole could itself stand as a singularly appropriate metaphor for the way that discussion stalls whenever it attempts to move beyond the desktop metaphor. The provisional status of this metaphor gradually becomes solidified so that it moves from being merely what 'we're currently considering' (2.3: 8) to the definitive field of application:

(2.3: 23)

NB: Right. Well I think that this would stand, but we need an extra paragraph in here which says the icons are only for the desktop metaphor.

In this instance, the resolution of doubt as to the scope of the standard is achieved by default, and can be clearly related to the partial dependence of HCI discourse on existing designs, as discussed in the previous chapter. I now want to look at other means by which such resolutions are accomplished.

7.6. Managing 'the horrors'

Thus far, the emphasis has been placed on the manifold uncertainties that confront participants in the work of trying to specify standards. The issue of representational adequacy has been presented as a pervasive one for the committee, in the double sense that it constitutes both the topic of discussion and a fundamental obstacle to discussion. And yet, for all that issues are raised that are irresolvable in principle, decisions are reached, definitions are developed and in general, progress is made. The ways in which this is accomplished are the focus of this section.

In a general sense, this management of indeterminacy can be characterized in orthodox ethnomethodological terms as practical reasoning, that is, as a pervasive feature of all discourse[12]. However, it is suggested here that this has a more specific significance. The uncertainties that surround the attempt to define even the most basic terms and concepts can be related, at least in part, to the relatively young state of the field of HCI, and to the relative novelty of graphical computer interfaces: the use of icons is generally seen as beginning with the Xerox Star interface at the beginning of the 1980's (Smith et al, 1982). Defining a standard therefore necessarily involves not only an attempt to prescribe good design practice, but also, since such prescription largely entails extrapolating from existing design, an attempt to stabilize and fix the meanings of terms and concepts where such meanings are still subject to much discussion and dispute. In the terms that this thesis has adopted, this is part of the process of a new discipline's establishing its own discourse; that is, defining a legitimate and distinctive field of study and specifying the objects that lie within it. Of course it is not possible to predict with any confidence the impact that the work of this particular group will have on the future shape of HCI discourse; in this respect, the argument put forward is subject to the same problems of representational adequacy as is the work of the standards group. But it is claimed that the

---

[12] See for example Woolgar's spoken comment in Ashmore (1989): 'I think that talk as we use it [..] is, um, like its main property is a way of, of rendering oneself immune from methodological horrors' (Ashmore, 1989: 176).

work of this group can be fruitfully regarded as exemplifying aspects of the process by which such a discourse is established, whilst it is also germane to note that it certainly aspires to this kind of impact.

The management of uncertainty, and the achievement of progress towards the definition of a standard can be described in terms of two different dimensions. On the one hand, there are a variety of procedural devices and conceptual distinctions which are brought into play during the meeting, notably by the chairman, in order to ensure that the business of formulating positions and making decisions goes according to schedule[13]. On the other hand, any decisions and formulations that are reached at this particular meeting lie within a historical and textual continuum: in order to make progress any positions, formulations and definitions should, wherever possible, be consistent with the work of past meetings, and made with an eye to probable future work. There should also be consistency across different documents that are produced concurrently. Taken together, there is therefore a tendency for positions that are outlined at one meeting, which may be provisional and adopted more for procedural reasons than as a result of consensus, to acquire a solidity as subsequent work reiterates and develops them. The establishment of a position is then a social process, both in the conventional sense that its formulation is the product of a particular form of social interaction, and in a more specific sense put forward by Latour (1987), that its credibility and authority derive from the number of associations and connections that can be built between texts.

An obvious instance of an issue which can be resolved by explicitly procedural means is how to ensure that a coherent national view emerges from a meeting in which, as we have seen, a variety of different constituencies and viewpoints are seen as being represented.

---

[13] This is not to claim of course that such procedural devices are peculiar to the work of this committee.

(2.3: 24)

NB: Well, maybe this is the point where we should have a .. decide, well yeah, we should take a UK view on this. Do, does the UK believe that

The formulation of a national position here is explicitly orchestrated by the chair by signalling to participants that at this point they should address an issue - whether an icon is distinct from the category of screen symbol - as national representatives. This puts much of the group's discussion of other national positions into question, insofar as there was a tendency to assume coherent essential positions underlying the various documents and statements in which they were formulated, for here the production of a UK view is essentially a practical procedure: following Garfinkel and Sacks (1970), it could be characterized as 'doing' taking a UK view, that is, put loosely, as a situated speech act. The necessity for such explicit orchestration can be seen in the chair's remarks which immediately follow, on whether the word icon can be restricted to a specific category given that 'we, which is, as I understand, well perhaps they can speak for themselves, which, some parts of the industry use that definition' (2.3: 24). The problem of representational adequacy in terms of representatives is therefore resolved by explicit procedural means.

A key procedural method for dealing with other irresolvable disputes is the invocation of time constraints; there are numerous instances of this throughout the transcript. The following extract occurs at the end of a lengthy discussion as to how much variation the standard should allow. The example that is under discussion - although as we have seen there is considerable flexibility on whether such icons are examples of a wider class of objects or the substantive topic of discussion - is the possibility of allowing both a waste basket and a waste bin as legitimate alternatives, with especial reference to the problems users might have in recognizing one if they were familiar with the other.

(2.3:16)

TC: The question is, if you have an expectation that there is a deleting object on there, do you immediately say oh that must be it, because if you come from a Mac {to a} similar system with a waste paper basket, will it give you any problems?

YR: I think a lot will depend on where it's placed as well, cause you, I mean it's like the car, you expect er the indicators to be at a certain place

NB: Well, this discussion's going on rather a long way,

YR: Yeah

NB: are we reaching any consensus do you think?

The chairman here intervenes to curtail further discussion. The point at which this takes place is significant. It is certainly not the case that the timing of intervention reflects any progress towards an agreed position in the discussion. The chair's statement, 'Well, this discussion's going on rather a long way, [..] are we reaching any consensus do you think?' can be read as suggesting that as the discussion becomes more extensive, it moves further away from the business at hand of agreeing a position. The implication is that the issues being raised are not relevant to this essential work. However, the interpretation that I propose is different. The discussion has not strayed into irrelevant areas so much as opened up areas of considerable and fundamental uncertainty. The preceding discussion had mainly related the problems of recognition to the inherent features of two different icons, and there had been little consensus even within these parameters. But at this point, a further element is introduced: the speaker YR suggests that the intelligibility of an icon is also related to its location as well as to its inherent graphical properties. This introduces a further, potentially complicating, set of factors to the discussion: if the intelligibility of a given icon is also a function of its spatial relation to other objects on the screen for example, arguably this might throw into question the feasibility of the whole project of specifying standard forms for icons considered in isolation. The intervention then prevents the discussion engaging with further uncertainty in this direction. One might say that in principle the issue raised is of vital importance to the work of defining the standard; it is only in practical

terms, given the importance of producing some sort of output at this meeting, that the chair is correct to imply its irrelevance to the business at hand, since it introduces difficulties that might not be resolvable within the frame of reference of the working group. The chair however would be unlikely to concur with this interpretation, since it suggests a wholly pragmatic approach to generating decisions that belies the apparent intentions behind assembling experts in the field to discuss icon standardization.

This distinction between principle and practice is, as we have seen throughout, a cornerstone of ethnomethodology's formulation of practical reasoning. However, just as it was argued in chapter 5 that 'context' cannot be used as some kind of pre-interpretative bedrock for ethnomethodological analysis, this distinction between principle and an opposite, such as practice or pragmatism, can also be reflexively put into question, since, as we shall see, it is a flexible distinction that can be and is used by participants in order to achieve certain rhetorical effects. Again, the reflexive implications of the material presented in this account have the potential to deconstruct the credibility of the account itself. 'Principle' is employed in the following sequence, in which two drafts of a document are being compared, and the chair wishes to reach a swift decision on which is preferable, before the details of each are discussed:

(2.3: 23)

NB: Ok. Well let's see if, how we can draw any general conclusions fairly quickly.

[..]

NB: Well yes, the comment we might make to Simon is we don't like your draft, we prefer the original one, um, I'm just not you know I mean, that would be a suitable comment, it may not be a {}, but I think we should decide in principle if we think what Simon's done is moving in the right direction, even if don't have time to go {}

This brief sequence, comprising two utterances made by the chair within a short time span of less than a minute, again illustrates the rhetorical use of temporal constraints to effect and justify the procedural management of uncertainty and production of an agreed position. In the first utterance, it is emphasized that there are time constraints that prohibit detailed discussion, and that conclusions will therefore have to be general and quickly made. In the second, time limits are again mentioned, but the possible inference that could be drawn that any discussion cannot therefore be thorough is discouraged by the suggestion that they should 'decide in principle' which is the better draft. There is no explicit comparison with an unprincipled decision here, but the rhetorical effect, though implied, is clear. Elsewhere the distinction between principle and pragmatism is more explicitly invoked. The letter to the chair which forms a part of, and a commentary on, one of the documents that was pre-circulated, contains a classic formulation of this distinction in that it spells out the danger of being seen as unprincipled. The point is made with reference to some non-mandatory guidelines which formed part of an earlier provisional draft for a standard:

(2.3, Document 100: 2)
The problem with this is that the guidelines will be *ad hoc*, representing only what has been thought of at a meeting, since there has been no time to research the optimization of icon designs, and correspondingly unprincipled.

The crucial phrase here is 'only what has been thought of at a meeting'. Principled positions evidently aspire towards what might be called a transcendental significance in that they are seen as not reducible to the situated practical decision-making work that produces them. From a pure ethnomethodological standpoint this is untenable, but the point I wish to make is not that this writer is incorrect but rather to observe that the point of interest is that acknowledgement of the situated character of decisions is seen as a denigration of their integrity: they are merely '*ad hoc*'. This view is put into use in the service of arguing for the omission of certain material - non-mandatory guidelines - from the standard. The distinction between principled and *ad hoc* decisions is therefore used for specific practical purposes in this document: as a rhetorical device

for arguing against the inclusion of some material. This is also the case whenever a distinction between principle and some implied or explicit opposite is used in the meeting: we have already noted its use, in conjunction with references to time constraints, in order to justify the suggestion that discussion of a particular document should be at a specified level of generality. It is worth noting that it is used throughout the ensuing discussion of this document in order to make judgements about the significance of different contributions. Discussion is closed in the following way:

(2.3: 30)

NB: Well perhaps I'll throw it open, are there any issues of principle other than editorial detail that anybody has about the rest of .. does everybody, does everybody accept that, in principle that Simon has improved the document

The distinction here between principle and detail, especially in the light of the uncertainties that are a prevalent feature of so much of the discussion, is clearly a very useful device for managing the horrors. The further claim being made here is that such distinctions are practical accomplishments rather than inherent features of the topics of discussion; the distinction between principle and practice is a practical one. A final example will show its instrumental and flexible character more clearly. A discussion about the intelligibility of icons in different cultures at one point turns to the question of different types of folders that are in use in America and Europe.

(2.3: 10)

TC: The Germans, the Germans have box folders, box

JE: Surely they have filing cabinets?

NB: Yeah but you see shape of these um

JE: Oh sorry yeah {}

TC: No it {seems} both um

NB: Well, ok, what if we stand, if we, one goes in the standard, should the standard then prohibit you from the other? That's the ..

TC: Well when you count up the numbers, well look, ninety million or something in Germany. (laughter)

NB: {} No the point I'm trying to make is a matter of principle, does the UK believe that, whatever decision process we adopt, one of those, or having or should the standard have a mechanism to permit both .. as an example of the general problem?

Here, the suggestion that a decision should be based on the relative populations of different markets is dismissed on grounds of principle. It is not entirely clear to what principle is being contrasted in this instance. The strongest implication is that the suggestion is too closely tied to what the chair sees as a mere example, but it may also be that the legitimacy of basing the decision on purely market considerations is being questioned. In either case, the contrast is not a self-evident one to judge by the rest of the discussion. The former reading can be related to the frequent uncertainty already noted as to whether specific icons being discussed should be seen as substantive topics in their own right, or merely examples. Furthermore, even where there is recognition that discussion of a particular icon serves the purpose of exemplifying a more general issue, the point at which such an example could be said to be too specific to be of general explanatory value can never be clear cut but could itself be a matter for debate. The latter reading can be related to the problematic relationship of this committee, and HCI in general, to the computer industry and the market place. If this is the implication, it is not the case that the chair rejects the validity of such an orientation *per se*, for shortly before he had stated that a reasonable procedure for specifying standards that are intelligible in different cultures is to say that if there are 'good contenders in the market place, and these symbols work quite well in a wide range of cultures, then let's go with those' (2.3: 9). Whichever way it is interpreted, the contrast between principle and example, or between principle and market derived considerations,

is not therefore given but a discursive construct which is used for the practical purpose of managing uncertainty and shaping the decision making process[14].

The other side of the process of managing uncertainty can now be examined: that is, the location of decisions and formulations within a textual and historical continuum. The work of previous committee meetings, and the documents that are produced as a result of and in conjunction with them, form a framework which acts both as a constraint and as a resource for current work. This is significant in that it enables progress to be made without having to settle issues of theoretical difficulty. One extract that has already been quoted illustrates this well:

(2.3: 23)

NB: Well yes, the comment we might make to Simon is we don't like your draft, we prefer the original one, um, I'm just not you know I mean, that would be a suitable comment, it may not be a {}, but I think we should decide in principle if we think what Simon's done is moving in the right direction, even if don't have time to go {}

Part of the business of the meeting is to look at a new draft of a document on icon standards (Document 106). This is assessed not so much in terms of its adequacy in relation to an ideal, since as we have seen there is considerable doubt as to what such an ideal would be, but by means of a comparison with a previous draft (Document 100). Thus a judgement can be made on whether this work is 'moving in the right direction', whether progress is being made, without having to resolve problems of detail. Elsewhere the work of previous meetings can be used to settle points of uncertainty. A question which is posed in terms of the scope of a conceptual category, the

---

[14] This is not to imply that such use is unprincipled: indeed the reflexive implications of the argument presented here prohibit such an assertion. The rhetorical use of 'principle' to assert disciplinary competence within both HCI (Long, 1989) and sociology (Norman and Thomas, 1990) has already been noted in chapters three and four.

desktop metaphor, can be answered not in theoretical terms but by referring to the fact that a decision had already been taken on it at a previous meeting:

(2.3: 7)

{TB}: Would the CAD market fit into the desktop metaphor? {}

MP: Well possibly (laughs)

TC: We, we excluded that at an earlier meeting I think. {} back here

Clearly the existence of this previous decision is a resource of considerable utility, for the question is one of many raised for which there can be no hard and fast answer. It is a resource which enables the avoidance of lengthy discussions on issues which may defy resolution. More generally it can be used as a rhetorical device to justify a position without giving the kind of reason that is asked for. A further example of this is the following:

(2.3: 26)

YR: What's, what's the objection to having a generic, using icon as a generic term rather than you're sort of saying it should be specific to

NB: Well we had a lot of disagreement as to what it is and if, we'd just like to {take} towards a plausible proposal which is both technically easy to define and consistent with normal usage I suppose.

This reply avoids stating the objection to using icon as a 'generic term' that the first speaker had asked for, but instead points to disagreement in the past and the kind of proposal that they are working towards. Here not only is the work of previous meetings referenced, but also the significance of any decision that may be taken on this issue is related to its future use: the 'plausible proposal' being that which would be made at the next international meeting for which this one is, in part, a preparation. In another instance, speculation on possible future work

on other standards is used to affirm the advisability of a decision to include 'tools' within the definition and scope of the term icon, but to exclude those functions referred to as 'control':

(2.3: 27)

NB: It's also relevant {} standard, I could quite implicitly might, {} trivial, it's merely another application {} something which is defined elsewhere, you wouldn't think they're likely to standardize control *per se*, you might {}, you're much more likely to consider standardizing tools, and therefore if you've got a standard for icons which may have a {part} on tools

Since 'tools' are more likely to be standardized than 'control' in the future, this makes their inclusion in the icon standard desirable. It should be noted that this occurs towards the end of a lengthy discussion on the scope of the term icon which has been on the level of categorizing different classes of objects in conceptual terms. Possible future work therefore provides a practical resource for adjudicating on such a discussion in the same way as does the existence of previous decisions.

The most significant aspect of this process is the production of documents. It could be said that they are the medium through which decisions and formulations are transmitted in a concrete form from one meeting to the next. However, this would be both to understate their importance and to misconstrue the role that they play. In the first place, all aspects of the business of the committee can be understood as being directed towards the production of documents, which forms the *raison d'etre* of the working group. Moreover, the implication that texts simply record what has been decided at various meetings neglects the significance of the reflexive ties between decision work and document production: for example, the need to produce a plausible written proposal at the next international meeting has a decisive effect on the discussion which takes place. Here, the discussion is specifically oriented towards this task even though it consists mainly of defining the limits of a particular category on grounds of principle. Elsewhere, past decisions inscribed in documents can be invoked to avoid the necessity of resolving uncertainties.

The process is a textual one. A final example demonstrates the extent to which the UK position is a textual product.

(2.3: 23-24)

NB: We'd better leave it to you to find some words to say what they are

MP: Yes, ok

NB: It might be, if, we'll come to this in a minute, but whatever terminology, the terminology in here ought to cross-reference your terminology

MP: Yeah, it was closer in the original where it mentioned I think a whole host of things which are these sub-categories (of these) umm, object, process, action, things like that

Here the suggestion made by speaker NB is that Document 106 should be explicitly correlated with an already existing document (Document 107, drafted by the speaker MP) in order to achieve consistency in the UK position. This provides the grounds for an evaluative judgement on the relative merits of this section of Document 106 and its previous draft: the original is preferred because its terminology correlates more closely. It has already been noted that the UK position is in large measure achieved by procedural means; it is now evident that it is also a literary production. Indeed the latter aspect is probably the more significant in that it is through inscription that definitions and terms acquire solidity, no matter how provisional the initial decision may have been. In this instance, two documents are explicitly correlated in order to produce a more consistent and therefore credible position. But it is also the case that deciding to correlate the two documents has the effect of deciding between different definitions on the basis of their coincidence with the definitions in the already existing document. In this respect document production, or what Latour has called a process of 'inscription' (Latour, 1986), is perhaps the most important element in shaping the course of decision making work.

7.7. Conclusion

To briefly reiterate, it has been argued here that the work of this group can be fruitfully characterized in terms of its management of fundamental uncertainties in the attempt to prescribe standards and principles for design. These uncertainties centre upon the pervasive issue of representational adequacy, which manifests itself as the topic for discussion, as an obstacle to discussion, and even as a rhetorical resource. Such uncertainty was tentatively related to HCI's status as an emergent discipline whose terms and concepts are relatively unstable. The management of these uncertainties by procedural, rhetorical and textual means was in turn described as being part of the process of establishing a discourse, of solidifying its terminology and specifying the objects within its domain.

There are parallels between the view of standard specification that has been outlined here, and some of the work in the sociology of scientific knowledge (SSK) on the ways in which facts are established and agreed upon in scientific work. The parallel cannot be pressed too far, since the process of social negotiation that takes place in this setting is relatively explicit in comparison with, for instance, experimental work in the natural sciences. However, if the sceptical stance that much SSK takes towards the idea of the 'discovery' of facts in the natural sciences is accepted (Woolgar, 1976; Brannigan, 1981), the distance between the two processes is not as great as might be supposed, given that the emphasis in such accounts falls on the social processes whereby a consensus is reached on what is and is not to count as a particular factual discovery. One aspect of this approach which is of especial relevance here is the frequent injunction to examine the work of scientists at moments in time before consensus has been reached on what constitutes a 'fact': Collins (1981), for example, argues for the importance of studying controversies before they are resolved. The significance of this approach is that it illuminates the contingent and constructive nature of the processes by which facts come to be established; as such, it is anti-realist in that it sees knowledge arising not from the logic of the material being studied, nor from the cognitive processes of the scientist who tries to apprehend it, but rather as a cultural product.

In the same way it has been argued here that, from an analytical point of view, a significant feature of the work of this standards group is its location at a point in time when many of the terms and concepts of HCI discourse are relatively provisional, and when its own prescriptions for present and future design practice have not as yet been agreed. Whilst the specification of a standard is not identical to the establishment of a fact, there are important similarities. Firstly, as already mentioned, both can be said to involve processes of social negotiation. Secondly, the consequences of both may be similar in that, in both cases, future practice is constrained[15]. In this respect, Foucault's view of discourse as encompassing material practices and artefacts is of particular relevance (Foucault, 1977); whilst the term has been used in this chapter in its more usual purely linguistic sense, the fact that decisions made by this and other national bodies may become embodied in the design of artefacts makes this more inclusive view an apt one[16]. Viewed from such a perspective, this analysis of standards specification may not be equivalent to a deconstruction of scientific facts but it does share the same anti-realist impulse: it argues that the decisions that are reached and the positions that are formulated derive from a variety of *ad hoc* means, pragmatic considerations, arbitrary conceptual distinctions and, perhaps above all, from the contingencies and requirements of document production. Therefore, having stressed in previous chapters the significance of a discipline's existing discourse for its practitioners, and thereby pointed to the constructive character, in this chapter I have attempted to show that the process by which such a discourse is itself constructed is also worth examination: the important proviso being that such constructive work must always take place within an existing framework

---

[15] 'Fact' is being used here in a sense which is probably only appropriate to the natural sciences. If HCI is viewed as an 'artificial science' (Simon, 1981), it may be that specifying standards for design is as close to establishing facts as is possible, since the paradigm for such a science is one of creating artefacts rather than discovering facts.

[16] Pinch and Bijker (1984) argue that Collins' (1981) method for analysing the stages of scientific knowledge, with special emphasis on the controversy stage, can be directly applied to the construction of technological artefacts. Latour also argues for the general applicability of strategic timing in studying scientific and engineering work 'before the facts and machines are blackboxed' (Latour, 1987: 258)

of discourse[17]. The two emphases are complementary: they both entail a sceptical theory of language, or more generally, representation with respect to its object.

The problem raised by any sceptical or deconstructive account of representation is that of the representational adequacy of the account itself. Any serious use of concepts such as the 'methodological horrors' must acknowledge the issue of reflexivity, in the sense that the phenomena described have implications for the act of description. For instance, the distinction between principle and practice that is key to the notion of 'practical reason' that has informed the analysis was put into question in the light of the description of the rhetorical uses of such distinctions by members of the working group. More generally, the problem of representational adequacy has been presented as a problem for those being studied, whereas it is also of course a fundamental one for the observer: for example, what is the warrant for making statements about the discourse of HCI on the basis of utterances from individuals whose ties to other constituencies (companies, institutions, countries) could be equally significant? My position here is that it is neither possible nor desirable to circumscribe the effects of scepticism in order to safeguard the account in which it is formulated; but also that, whilst this has implications for the claims that can be made for the account, it does not imply a lack of validity. This issue is addressed more fully in the final chapter.

---

[17] This formulation skates over a number of difficult methodological problems which are addressed in the final chapter.

Chapter 8

Conclusion: representing discourse

This thesis has presented an argument about the substance, character and structure of the discourse of HCI, and its significance for an understanding of disciplines and their inter-relations. Although this argument has been largely constructed from the implicit viewpoint of a detached observer, whose viewpoint is (metaphorically) somewhere above the phenomena described, it of course remains the case that the account given here is itself located within a particular theoretical tradition, a particular discourse; it is therefore in no sense disengaged from what it describes. Accordingly, questions relating to the representational adequacy of the thesis itself form the main critical focus of this final chapter, which is organised as follows: the main points of the argument are summarised and clarified; the changing state of HCI discourse, which stands as a challenge to some of the assertions made here, is discussed; methodological and theoretical questions about the status of the account, in particular those implied by the account itself, are then directly addressed; finally, some directions for future research are indicated.

8.1. The argument

The theoretical orientation of this thesis needs no reiteration, but it should be stressed that the conception of language as constitutive, not reflective, has been fundamental throughout. Drawing

upon some ideas of Foucault's on the disciplinary organization of knowledge - ideas which have seldom been used in the social study of science - and upon ethnomethodology, I developed an approach which treats disciplines as distinctive and formative discourses. This approach was used to analyse the discourse of HCI in order to identify its some of its key objects, parameters and assumptions, and to critically address questions about the feasibility of interdisciplinarity in general, and the role of sociology in particular, within the field as a whole. The formative character of discourse that is postulated also directs our attention towards the distinctive discursive character of the account that I have given; for although some of the argument takes the form of an overview or survey, these visual metaphors are misleading in that they imply that I am 'just looking' at HCI. Rather, it must be recognized that the 'gaze' employed throughout is itself formed within a particular set of theoretical assumptions.

It has been argued that the discourse of HCI can be described in terms of two dimensions. In the first place, it claims to represent the user. This can be read in two senses: it adopts the role of the user's representative, and it constructs representations of the user. Representations take the form of cognitive models, broadly defined: that is, they are representations of an individual's inner mental processes. The legitimacy of HCI's role as user's representative derives largely from its claimed expertise in cognitive representation. In the second place, it has a marked orientation towards what I have called 'the performative'. Again, a double meaning is intended: there is an emphasis on the need to come up with practical and directly implementable knowledge, with a corresponding mistrust of theoretical work, and a general commitment, sometimes explicit, sometimes tacit, towards improving performance and efficiency in the office.

The structure of this discourse, and the interplay of these different dimensions, have been interpreted in the light of a particular view of disciplines and their formation which stresses the significance of disciplinary alignment and self-assertion. A discipline's discourse - which, following Foucault, encompasses practices and artifacts - is an attempt to constitute a distinctive domain of objects for itself, thus establishing its own legitimacy and demarcating itself from other disciplines: some central objects of HCI discourse being 'user', 'user interface' and 'usability'. Claims to represent the user, often expressed in a markedly compassionate rhetoric,

and their sometimes uneasy co-existence with the commitment to efficiency, can be understood in terms of this assertion of the need for the discipline's services. The discipline's position within the larger enterprise of designing and building software is also significant for the character of its discourse. It claims a distinctive role within this configuration through its representation of the user *to* the designer; yet, for all that its purpose is to change and improve design by insisting that the user be taken into account, it has a relatively indirect relation to design practice itself. It has to attempt to move from the description of existing designs to the prescription of future ones. There are however a number of problems with this, not least the communication between designers and HCI practitioners: an increasingly significant proportion of work in HCI is devoted to the ways in which its impact on design can be enhanced, as is discussed in the following section.

I have argued that as HCI is still an emergent discipline, the process whereby it attempts to assert itself and establish its own discourse is still discernible. In practice however, the parameters for potential contributions are in place. Thus, for all its multidisciplinary aspirations, work from disciplines which might offer radically different perspectives tend to fall within these existing parameters. This is the case with sociological work to date. It shares a focus on understanding the cognitive individual, albeit in a different way, and it has not questioned the discipline's performative criteria: for example, the rhetoric of user-centred design is largely taken on trust, and the concern with efficiency is never addressed[1]. More fundamentally, it shares with other work within HCI a realist epistemology; and this seems to be partly connected to the perceived need to supply solid 'findings' in an implementable form.

My own analysis of some human-computer interactions showed the extent to which the form of existing work is not a necessary one for sociology, by providing a different account. The focus was

---

[1] My own scepticism towards this rhetoric may be easily misread. It is neither a criticism of the sincerity of individuals, nor a denial of the need to, for example, design safety-critical systems with more emphasis on questions of legibility and so forth. Instead, I am interested in the dynamics of disciplinary growth: on this level, one has to look at the work that such a rhetoric does, regardless of its plausibility or apparent worth.

placed not on the individual user's reasoning, mediated by social circumstance, but on language: instead of reading discourse as an indicator of mental phenomena, as in cognitive psychology and much sociology in the field, it was analysed as a constitutive element of social interaction in its own right. The analysis was therefore neither 'user-centred' nor 'human-centred' but 'language-centred'. Beyond showing how a different analytic focus was possible within a sociological account, it also suggested that a more sceptical form of sociology could contribute something of value to the field. Although the stress on interpretative flexibility precludes presenting results as 'findings', this very flexibility can be used to question prevailing conceptions within the field such as conceptions of the user, interaction, and social context; in the case of social context, the reformulation having direct implications for attempts to incorporate and implement an impoverished notion of 'context' in predictive models of interaction.

The analysis, then, has attempted primarily to shift the focus of attention from the phenomenon of human-computer interaction, as it is given within HCI discourse, towards the form and content of HCI discourse as a topic of interest in its own right. The critical distance established by this shift was also used to enable the reformulation of HCI's object of interest, interactions with computers, in different terms: the latter serving to reinforce the assertion that the discursive structure of HCI has a formative effect on sociological work which aspires to make a valid contribution on the discipline's terms.

A number of wider claims are implied in my argument about the relations between disciplines, and the problems of interdisciplinarity in general. The constitutive character of discourses proposed here suggests that the realist conception of each discipline providing a different perspective on a common object is untenable (cf Woolgar, 1989). Furthermore, in a multidisciplinary field such as HCI, the discourse as a whole is structured around a set of assumptions that are derived from a core discipline: in this case, cognitive psychology. It is these assumptions that have been particularly significant for sociology; contributions have come from those forms of sociology that tend to share them, and where there is difference, the move to a new discursive field gives fresh connotations to sociological work.

However, we also saw that the discourse of HCI is not set in stone, and that the work of establishing, regulating, and solidifying a discourse was still evident; moreover, that this work could be construed as dealing with the severe problems of representational adequacy that are attendant on attempts to prescribe, in realist terms, standards and specifications for uncertain, since as yet unspecified, objects. I contend that, just as work within the sociology of scientific knowledge's focus on scientific knowledge prior to the formation of consensus is used to support its view of the constructed nature of knowledge, the study of standards committee work can similarly be employed to support my argument for the constructive character of discourse, which forms the basis of the thesis. It also implies though, that whilst I have discussed HCI discourse as an entity with identifiable features, these may be subject to change. Accordingly, I now turn to consider the dynamics of current change within HCI before moving on to a discussion of some of the implications of this and other methodological questions for my analysis.

8.2. The dynamics of change: HCI and system design

A major difficulty with the approach taken here is that formulating discourses in terms of their objects and structures necessarily gives a synchronic, or static picture of the discipline. Whereas, as I suggested in chapter seven, the discourse of HCI, particularly given its recent emergence, is still undergoing significant changes. Perhaps the best way to read my discussion of the discourse of HCI is as (to use a realist metaphor) a snapshot of a process at one point in time. To give a better sense of this process, I will briefly consider the question of the changes that can be currently discerned within HCI, and some pressures for change that may confront it in the immediate future; the issue that underlies all of these is the problematic relationship of HCI discourse to the practice and achievements of system design.

I have stressed the significance of HCI's relationship with the practice of software design for the character of its discourse; in particular, that HCI has at best an indirect impact on the design process. HCI has always endeavoured to represent and communicate the needs of users to software designers and engineers; recently however, there has been a discernible shift of focus onto the design process itself. The problems of how to communicate HCI knowledge to designers,

how to support design practice, or alternatively, how to integrate HCI practice within the software development life-cycle are moving towards the centre of the discipline's concerns. Recent HCI conferences, for example, have devoted a large proportion of their time to these issues.

The extent of the importance afforded to this problem of 'technology transfer' can be evinced by looking at a shift in the use of one of the discourse's key terms, 'usability'. In a sense, this is understandable as a general term which is in principle applicable to a variety of phenomena; however, in practice, its application has been restricted to the user interface, or software system more generally, seen from the point of view of the user. Recently, it has been increasingly employed in a different context to describe the adequacy of information that is passed from HCI practitioners to designers. For example, Young describes one of the criteria for assessing his simulation models as 'usability: are the models practical for the designers who are the intended users?' (Young, 1990: 1056). The designation of designer as user here also contributes to the shift in orientation: the conceptual scheme of usability in relation to intended users has been preserved intact, but re-orientated and refocused on HCI output: extending this model in HCI terms, the designer interface is substituted for the user interface.

A particularly striking example of this shift can be found in de Souza and Bevan (1990)[2]. They discuss an experiment in which a number of designers were asked to use a draft standard of some design guidelines in the course of redesigning a menu interface. The results suggest that although designers had difficulty interpreting the guidelines, the redesigned interfaces conformed with their recommendations to a large extent. The authors see these results as implying that there is a problem with the format of the guidelines, since existing rules for the production of standards place the stress on technical accuracy and therefore 'make it difficult to present the information

---

[2] There are numerous instances of the application of 'usability' to HCI output: for example, Forrester and Reason (1990: 283) where the complexity of the material given to designers by HCI practitioners is ironically compared with the principles that it is supposed to be promoting. It is also implicit in much of the standards committee's discussions, described in chapter seven.

in a manner which optimizes usability' (de Souza and Bevan, 1990: 440). Usability is applied to design guidelines, in opposition to 'technical' concerns, in a manner that replicates its use in relation to the user interface. The recommended solution continues the replication: improving the usability of guidelines through a process of 'iterative development' with designers to identify and rectify 'errors and difficulties of interpretation' (ibid: 440)[3].

This paper is also of interest because it graphically demonstrates the rather patronising attitude towards designers and their skills that is implicit in many of HCI's more recent moves towards 'supporting' or 'empowering' the designer[4]. One reasonably obvious interpretation of the results that is never even contemplated in the paper is the possible irrelevance of the guidelines to the design process, even though designers had difficulty with 91% of the guidelines, yet only violated an average of 11% of them; for this suggests that design may be effectively carried out without reference to a formal and explicit set of injunctions which have, in any case, been derived from existing examples of design. The problem however is perceived as a purely technical one - how to make design practice conform to the guidelines more closely - which could be remedied by more fully explicating and specifying the way in which a guideline is to be interpreted, thereby overcoming what is seen as the underlying cause of the problem: that designers use their own intuitions and interpret guidelines 'in the light of their own experience' (ibid: 440)! The mistrust of any kind of craft skill that has been noted as a feature of the discipline is apparent here, as is the certainty that it is desirable to find ways of making designers evaluate designs against explicit guidelines: it is suggested that if a measure of conformance to the standard can be devised, this will provide a 'powerful incentive for designers' to do this (ibid: 440).

The need to have more of an impact on design practice is recognized throughout HCI as being important for the discipline. Whilst HCI has always been concerned with design issues, there is a growing consensus that it must focus more on the problems of making itself heard, understood

---

[3] The next area for regulation being, presumably, how to carry out iterative guideline development.

[4] This was alluded to in chapter six.

and valued by designers: but if the discipline takes upon itself the function of *policing* design, then the more likely outcome would seem to be that designers' resistance to HCI's ideas and prescriptions will increase.

The other side of the coin is the effect that the rapid pace of technological innovation will have on the discipline. If, as I have argued, HCI occupies an essentially reactive position with respect to design innovation, then the speed of change poses a challenge to its attempts to develop methodologies and principles. For instance, one of the talking points of the CHI '91 conference was a prototype system which used animated icons. Nowhere in the transcript of the meeting of standards committee on icons, the topic of chapter seven, was this possibility mentioned, even though considerable time was taken up with speculations about future developments in iconic representations: for example, the possibility of holographic three-dimensional images was discussed. This unforeseen development, if it were to gather any momentum, would pose serious problems for the work of this committee and mitigate against the feasibility and effectiveness of its attempts to devise iconic standards[5]. This is not a new problem for HCI[6]. The keystroke component of the GOMS model proposed by Card et al (1983), although widely respected as a pioneering and usable quantitative method, was arguably obsolescent at the time of its introduction, since it was only properly applicable to command-based interfaces: the development of direct manipulation interfaces has rendered it of limited value. Future innovations may however pose even more dramatic challenges to HCI, the much vaunted 'virtual reality' systems being an obvious example.

One can only speculate on how such innovations will affect the mainstream of system design (would a virtual reality database be advantageous, feasible or even conceivable[7]?), and how HCI will attempt to accommodate them, but it is reasonable to assume that changes in the

---

[5] It would also have implications for my analysis of the committee's work: see next section.

[6] Arguably, it is also a persistent problem for all standards committees working in the general area of information technology.

[7] I am told that such databases have been attempted, but have not been successful.

discourse of the discipline will result. It may be for example that the boundaries between HCI and system design will become blurred. Already, within the USA, technological innovation has a far more prominent place within HCI conferences than it does in Europe. Cognitive psychology may come to occupy a less central and definitional position within the discourse if such an integration takes place. It may also be that the growth of interest in computer-mediated communication, especially in the area of computer supported cooperative work (CSCW), will change the nature of the role that sociology plays, insofar as the interaction between people becomes more central[8].

These speculations direct our attention towards the dynamic and comparatively unsettled state of the discourse of HCI, and underline the point that the analysis that I have presented must be read as being temporally specific. Moreover, they also imply not only that changes may take place in the content of its discourse, but that the terms of HCI's very existence are not fixed: for its disciplinary structure and its alignments are themselves subject to change. This suggests that the theoretical model which I have employed may have certain limitations for the analysis of an area of discourse which is as volatile and unstable as this. Let us now move on to discuss the validity of the analysis in the light of this and other considerations.

8.3. Reflexive questions

8.3.1. The concept of discourse

A cornerstone of my analysis has been the concept of a discourse, derived from the work of Foucault. Whilst my use of this concept constitutes an implicit endorsement of its analytic value, it does have certain shortcomings which should be taken into account in assessing the validity of the argument that has been presented.

---

[8] Some would argue that the development of CSCW is also technology-driven, for all its rhetoric about responding to an already existing need.

The first problem, to which I have already alluded, is that it is a relatively static concept. A discipline's discourse is formulated as being complete and in place prior to the utterances and practices which derive their form and content from it. Thus, a process is reified into a static entity, with the result that it is difficult to account for changes that take place within the discourse; since HCI operates within a domain of rapid change, this may be a serious limitation. Part of the problem with Foucault's model here is that it is difficult to avoid formulating discourse as an entity which floats independently of the statements which it comprises; whereas it is perhaps better understood as a methodological device for understanding the relation of statements to the possibility space established by the relevant field of speech and writing to date. On this account, the relation is not determinate, but reflexive or dialectical.

The discussion of the standards committee endeavoured to overcome these shortcomings by arguing that an attempt to establish and stabilise a discourse could be read in its work. To do this however, it was necessary to loosen the theoretical framework so as to tacitly grant a degree of agency, albeit marginal, to the speaking subject in the formation of discourse. It would not be true to say that such a move is a pre-condition for adequately describe changes in a discourse: for example, HCI's interaction with other discourses has been shown to be particularly significant in this respect. Nevertheless, the articulation of process is made a more difficult task if the conventional agent of change, the human subject, is denied a central role[9].

---

[9] The *precise* relation of speaking subject and discourse is an extremely complex question: it is beyond the scope of this thesis to attempt to settle, in passing, what is a major and unresolved problem for sociology, and to a lesser extent, philosophy. It might be argued that ethnomethodology has more to offer here in terms of accounting for change, since it grants a more central role to the speaking subject and views cultural phenomena as the artful accomplishments of social actors; to cite an example from this thesis, the HCI consultant's utterances and writing might be said to conform with the discourse because she is concerned to produce work that is recognizably a genuine contribution from an HCI practitioner. However, this amounts to a re-insertion of conscious intention into the process, which (apart from being contrary to the theoretical position for which I have argued) is problematic on methodological grounds: how do we know that the consultant intends this conformity?

A further possible limitation of the notion of discourse concerns its applicability to contemporary knowledge, and particularly to a multidisciplinary field such as HCI. The concept was used by Foucault in the course of historical studies of the 19th century and earlier. However, Lyotard suggests that fragmentation, and the demise of traditional disciplinary boundaries are characteristic features of late 20th century knowledge (Lyotard, 1984: 39). It may be that this fragmentation challenges the validity of analyses built around the concept of unitary and discrete discourses. In the case of HCI, the complexities are considerable: I have described it as comprising a number of discourses, cognitive psychology, computer science and ergonomics, but conceivable as a single discourse in that a core discourse predominates. But we also have to consider that ergonomics is itself a conglomerate discipline in which psychology has a leading role; and that assumptions about the feasibility of representing many processes, human and otherwise, in computational terms cuts across parts of all of these disciplines in the form of cognitive science[10]. Moreover as I have already suggested, there is every reason to assume that the structural inter-relations of these different disciplines will be subject to considerable change. A model based on unitary discourses is put under considerable strain in such a domain.

Closely related to this is a more general and fundamental methodological problem about how to identify, or rather, designate a discourse and its boundaries. The problem is more general in that it applies to the analysis of any discourse, however traditional, but it is exacerbated by the complexity of this field. The interpretative flexibilities that must be negotiated are far more considerable than might be suspected from reading Foucault's magisterial and apparently comprehensive surveys. The designation of boundaries between HCI and other disciplines is an essentially constructive activity; it is not (simply) that the object of study is a complex configuration, but that even if it were perfectly mapped and comprehended, there would be no definitive criterion for deciding where a boundary was to be drawn. The standard ethnomethodological response here would be to see where participants draw the line, and to an

---

[10] Just as the general orientation to language that animates this thesis draws on ideas that cut across different disciplines in the humanities and social sciences.

extent this has been part of my strategy: but moving from resource to topic does not resolve the flexibility. Neither is the problem restricted to drawing boundaries: the implicitly holistic approach that I have taken towards the definition of a large and in many ways disparate body of work as making up a single discipline, the way in which my account has designated certain work as constituting a core discourse within this discipline, and the suppression of the significance of internal criticisms within HCI by describing them as peripheral to this core discourse, all attest to the active work of interpretation that underpins my representation of HCI discourse. It must therefore be conceded that a central pillar of my argument is a construct; however, it must also be stressed that since the constructive nature of language is that argument's central point, this concession does not imply, on the account's own terms, a lack of validity.

Put in different terms, this last problem is not a failure of this conceptual model *per se*, but another instance of 'the methodological horrors' which are a necessary feature of all interpretational work (Woolgar: 1983). Let us now consider in more detail the reflexive implications of interpretative flexibility and related phenomena for the credibility of my argument.

8.3.2. Self de(con)struction

Much of my analysis can be seen as having the potential to undermine itself, insofar as phenomena postulated and described within it have profound implications for the act of description itself. In chapter seven for example, the distinction between principle and practice, which was fundamental to the analytic framework, was seen to be interpretatively flexible in its use by participants, this flexibility serving to achieve rhetorical effects. In chapter five, it was argued that 'context ' should be seen as a construct, but that 'construction' characterised not only the work of participants but also the account in which this process was formulated. At a more general and fundamental level, arguing for the constitutive nature of language, for the distinctive and formative character of different discourses, and against realist theories of representation raises important questions about the status of the argument, and how it can be read.

Initially, this question of self-reference can be addressed by considering two different standpoints from which criticism may be directed at my account. The first, which might be a position taken by a critic with realist commitments, takes the form of what Ashmore has called a '*tu quoque*' argument (Ashmore, 1989: ch 3): this asserts that if, for instance, I argue that language is constructive not referential, my argument can be dismissed on the grounds that it too is merely a construct. My response to this is to take issue with 'merely': for the force of the objection derives from an underlying assumption that accounts should aspire to achieve certainty, whilst the position I adopt explicitly denies this. In other words, the implicit charge of inconsistency only holds if certain kinds of realist claims are made for the status of my argument; whereas I readily admit that the account should be viewed as a construct, but do not accept that a construct cannot be plausible, informative, persuasive, effective, or valuable in a number of other ways[11].

A second criticism, delivered from the standpoint of a commitment to scepticism, poses more difficulties. The charge here would be that reflexive considerations have been acknowledged in an essentially token fashion, and have not been integrated within the body of the account sufficiently; for taking reflexivity seriously has implications for the very practice and form of writing. For example, the use of the 'methodological horrors' as a technical instrument of empirical research is to suppress the instability that it introduces into the text in which it is used. The central issue here, or at least the one that concerns me, is intellectual rigour.

A response to this is suggested by Derrida's reformulation of Levi-Strauss's concept of '*bricolage*'. He notes that Levi-Strauss's consistent method has been 'to preserve as an instrument something whose truth value he criticizes' (Derrida, 1978: 284), and that this is made explicit in his discussion of *bricolage*, which can be defined as using the available means to hand, instruments which were not designed for the purpose for which they are employed[12]. Derrida continues: 'if

---

[11] Ashmore (1989: ch 3) deals with this form of argument in great detail, and articulates a number of responses besides the one briefly given here.

[12] *Bricolage* in its everyday use translates as 'do-it-yourself.'

one calls *bricolage* the necessity of borrowing one's concepts from the text of a heritage which is more or less coherent or ruined, it must be said that every discourse is *bricoleur*. The engineer, whom Levi-Strauss opposes to the *bricoleur*, should be the one to construct the totality of his language, syntax, and lexicon. In this sense the engineer is a myth' (ibid: 285).

This can be read as suggesting necessary impurity of any deconstructive account, for one is forced to employ (at least some of) the very concepts which are the object of criticism in the formulation of that criticism. If this is granted, then it can be argued that the question of how explicitly reflexive, or self-referential, a text should be becomes, if not arbitrary, then at least open; and the charge that a text's use of what it attempts to deconstruct constitutes a failing of rigour is subverted. In all matters of self-reference, conclusions are necessarily equivocal, and this one is no exception: indeed, Derrida's text can be used to argue for an opposed viewpoint[13]. I would not wish to argue, as should be apparent from the thesis, that the necessity of impurity implies that questions of epistemology and methodology can simply be ignored; but that claims for the superior rigour of more explicitly reflexive texts are questionable[14].

The further value of Derrida's statement for my argument is that it shifts the discussion from the consideration of individual texts to, in line with the approach taken throughout the thesis, the consideration of discourses, and in so doing suggests a further dimension to the question of reflexivity which brings us back to more substantive considerations about the relations between sociology and HCI.

8.3.3. Sociology for HCI? HCI for sociology?[15]

If the question of reflexivity is relocated at the level of discourses, it is transformed into a complex configuration of different questions about the relation of the thesis to the discourses of

---

[13] For example, he goes on to say that since the engineer is a myth, then the idea of bricoleur itself breaks down, since it derives its meaning from its difference from the engineer.

[14] Of course, rigour is by no means the only motivation for the development of textual reflexivity.

[15] cf Woolgar's distinction between 'sociology for AI' and 'sociology of AI' (Woolgar, 1985).

HCI and sociology, associated problems of identity and difference, and the issue of whether the thesis can be taken as a contribution to HCI discourse.

My analysis of the discourse of HCI may be read as critical account constructed from a superior vantage point, that is as a meta-level commentary upon the discipline. However, the apparent arrogance of this position is premised on the separation and difference of my discourse (or the particular sociological discourse from which it is formed) and the discourse of HCI. This separation is by no means given however, but a matter of some flexibility; consequently, the status of my account, and in particular the key question of whether it might be not only a sociology of HCI but also a contribution to it, can be seen as dependent on the way in which it is read[16].

If this thesis were to be taken as self-evidently a piece of HCI work, then it would suggest that the discipline was reflexive, in the sense of having an interest in examining itself and some of its pre-suppositions. (This would be contrary to my claim in chapter three that it had relatively narrow criteria for what constituted acceptable work, and placed heavy emphasis on practical and implementable work[17]). On this basis, the account of the discipline that I have presented would be a contribution to the discourse of HCI rather than simply an account of it. Conversely, if it is read as lying outside the discourse of HCI, it becomes simply a sociology of HCI, reaffirms the narrow discursive parameters that I have proposed, and questions the extent of HCI's multidisciplinary aspirations.

---

[16] I am leaving the account of human computer use given in chapter five to one side here, being primarily interested in the perceived status of descriptions of the discourse of HCI.

[17] Lyotard argues, by contrast, that contemporary sciences of all kinds are characterised by both 'paralogy', the continual attention to and undermining of a discipline's own premises, and 'performativity', in which the justification for scientific work lies in the generation of new ideas and investigatable problems alone (Lyotard, 1984).

The status of the thesis, following this logic, rests with its object of study. Moreover, the way in which it is read or perceived will itself tell us something about that object[18].

The inverse of this is the question of the contribution that the thesis might make to sociology. Lynch (1985) argues that it is important that sociology enters a new domain prepared to learn from it, instead of simply using it as empirical material to be fed into preconceived analytic schemes. I suggest that the novelty of the field is significant for sociology in two ways: firstly, it presents it with a new domain of phenomena (people and computers) which challenge accepted concepts of the limits of the social, and necessitates the development of new languages and methodologies for their study. Secondly, the experience of working in a multidisciplinary field is informative in that sociology's discourse takes on a different set of connotations, and different issues become paramount, as compared to those which predominate on its home turf: for example, we have seen that 'context' takes on a set of explicitly realist meanings in a domain which has been so far based around the laboratory experiment, and that questions concerning the precise relation of language to cognition are brought into a sharper focus given the concerns of the core discourse.

With this in mind, I will conclude by suggesting some directions for future research.

8.4. Directions for future research

A number of avenues for further research are suggested by this thesis. Given that assertions have been made and questions raised, not simply about the object of study, HCI, but about the form of sociological analyses of it, and for it, these avenues represent possibilities for developing the understanding of both disciplines.

---

[18] Given that the text in question is an unpublished thesis, this argument is an unavoidably hypothetical one.

In the first instance, let us consider the further understanding of HCI, assuming that the analytic framework used here is retained. The evolving state of the discipline to which I have drawn attention suggests that essentially the same research could produce a substantially different picture of the discipline at some future point in time. The particular focus would, I suggest, fall more on the relationship between technological innovation and HCI discourse. How will the ambiguities that characterise the relationship between HCI and design practice develop? How far will HCI remain in a reactive position with respect to innovation? Will disciplinary alignments change so that designers take a more prominent position within the discipline, and cognitive psychology a more peripheral one?

The development of interest in Computer Supported Cooperative Work (CSCW) can be seen as part of the development of an interest in a more 'social' perspective on the use of computers in the context of the place of work and the interaction between people. The overt concern here is to develop the use of computers to support and enhance collaborative work practices, to mediate and facilitate. This also implies a shift of role for sociology, in that social interaction (conventionally defined) is of more central interest. A number of directions are possible here therefore: contribution to the general aims of the project by offering an understanding of social interaction; critically assessing the terms of such a contribution; and studying the 'supportive' rhetoric, which implies a response to existing need, and considering whether this is being used to legitimate an essentially technology-driven process. As in this thesis, these need not be mutually exclusive approaches, and the interplay between them may be interesting in themselves.

Finally, in terms of sociological methodology, two key problems have been raised by my account. Firstly, the concept of distinctive discourses, whilst still valuable in presenting a view of the development of forms of knowledge, may have limitations: the association of a single discourse with a single, discrete, discipline may be outdated in a situation in which the fragmentation and complexity of disciplinary structures and alignments is continually on the increase. For example, currents of thought which cut across different disciplines - the computational metaphor, deconstruction, chaos theory - may be more significant than the traditionally defined

disciplinary context of a given piece of work. The field of HCI can here serve as a site for the refinement of the analytic model. Secondly, the phenomenon of human-computer interaction poses a challenge to a language-centred sociology: for all that it may consider the relations between human and computer as textual, or even social relations, there are no easy routes of access to its analysis. If computers, and more generally artifacts, are perceived to have an increasingly prominent function within social interaction (Latour, 1988a), then it is incumbent upon sociology to find ways of informatively describing their use that are not dependent on the presence of more orthodox interaction (talk).

If the majority of these suggestion are as much about directions for the development of sociology as for the representation of HCI, this simply affirms the need for sociology to continually reassess itself and its methodological shortcomings as it moves into a new domain. It is not a matter of concentrating obsessively on itself, but of attempting to be genuinely responsive to the challenges posed in that domain. In this sense, the problems that I claim are posed for sociological knowledge also tell us something about HCI and the use of computers.

Appendix

Data

Listed below are details of all the recorded and transcribed materials that are referenced in the thesis. Within the text an extract is referenced by the data reference number, and the page of the transcript; or, with documents, the data reference number, number of the document, and document page number.

1.1. (Recorded 26.6.89; video)

Evaluation of a prototype software package developed at the Open University, the 'Object Editor'.

Documents:

1. Informal report on evaluation

2.1. (Recorded 16.1.90; video)

User is shown how to produce a simple document using Microsoft Word 4 on a Macintosh.

2.2. (Recorded 27.2.90; video)

Evaluation of interfaces for the NPL project on adaptive systems; designer and consultant review current state of the system.

Documents:

1. Evaluation of the user interfaces for NPL project

2.3 (Recorded 4.10.90; audio)

Panel meeting of British working group IST18/-/9U on the definition of standards for icons and other screen symbols.

Documents:

96. Review of Stockholm meeting

100. S. H.'s draft document for icon standards

106. S.H.'s revised draft for ISO Stds

107. M.P's draft on objects and icons

2.4. (27-31. 8. 1990; audio)

Panel meeting from the Interact '90 conference on 'New approaches to theory in HCI: how should we judge their acceptability?'.

Transcription notation

Since my analysis has not been concerned with the minutae of conversational structure, I have not made use of a formal conversation analysis notation but have transribed materials into 'ordinary' text. However, a few textual devices have been used, and are explained below.

{}	Inaudible utterance
{word}	Uncertain transcription

..	Pause in conversation; in human-computer interactions may denote use of, or attention given to, computer
(word)	Description of events
[..]	Used in between extracts to denote omitted material

References

Agre P.E. (1990). Review of Suchman (1987). Artificial Intelligence, 43, 369-394.

Agre P.E. and Shrager J. (1990). Routine evolution as the microgenetic basis of skill acquisition.In Proceedings of the 12th Annual Conference of the Cognitive Science Society. Lawrence Erlbaum: Hillsdale, NJ.

Althusser L. (1971). Lenin and Philosophy. New Left Books: London.

Alvey (1982). A Programme for Advanced Technology: The Report of the Alvey Committee. HMSO: London.

Anderson P.B. (1986). Semiotics and informatics: computers as media. In Information Technology and Information Use. (Eds P. Ingwerson, L. Kajberg and A.M. Pejtersen.) Taylor Graham: London.

Ashmore M. (1989). The Reflexive Thesis: Wrighting Sociology of Scientific Knowledge. University of Chicago Press: Chicago and London.

Atkinson J. M. and Drew P. (1979). Order in Court: the Organization of Verbal Interaction in Judicial Settings. Macmillan: London.

Auer, P. (1990). Rhythm in telephone closings. Human Studies, 13, 4, 361-392.

Baecker R.M and Buxton W.A.S (1987). An historical and intellectual perspective. In Readings in Human-Computer Interaction. (Eds R.M Baecker and W.A.S. Buxton). Morgan Kaufman: San Mateo, California.

Bannon LJ. and Bodker S. (1991). Beyond the interface: encountering artifacts in use. In Designing Interaction: Psychological Theory at the Interface. (Ed J.M. Carroll). Cambridge University Press: Cambridge.

Barlow J., Rada R. and Diaper D. (1989). Interacting with computers. Interacting with Computers, 1, 1, 39-42.

Barnard P. and Harrison M. (1989). Integrating cognitive and system models in human computer interaction. In People and Computers V. (Eds A. Sutcliffe and L. Macaulay). Cambridge University Press: Cambridge.

Barnes B. (1981). On the 'hows' and 'whys' of cultural change (response to Woolgar). Social Studies of Science, 11, 481-498.

Baudrillard J. (1980). Forgetting Foucault. Humanities in Society, 3, Winter, 87-111.

Bazerman C. (1981). What written knowledge does: three examples of academic discourse. Philosophy of the Social Sciences, 11, 361-387.

Bellamy R.K.E. and Carroll J.M. (1990). Redesign by design. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Bench-Capon T.J.M. and McEnery A.M. (1989). People interact through computers not with them. Interacting with Computers, 1, 1, 31-38.

Bennett J.L. (1979). The commercial impact of usability in interactive systems. In Man Computer Interaction: Infotech State-of-the-art, Vol. 2. (Ed B. Shackel). Infotech International: Maidenhead.

Bennett J.L. (1984). Managing to meet usability requirements. In Visual Display Terminals: Usability Issues and Health Concerns. (Eds J.L. Bennett, D. Case, J. Sandelin and M. Smith). Prentice Hall: Englewood Cliffs, NJ.

Benyon D. and Murray D. (1988). Experience with adaptive interfaces. The Computer Journal, 31, 5, 465-473.

Benyon D., Murray D. and Jennings F. (1990). An adaptive system developer's tool-kit. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Benyon D., Preece J. and Rogers Y. (1990). A Guide to Usability. The Open University (in association with the Department of Trade and Industry).

Bilmes J. (1988). The concept of preference in conversation analysis. Language in Society, 17, 161-181.

Bodker S. (1989). A human activity approach to user interfaces. Human-Computer Interaction, 4, 171-195.

Bornat R. and Thimbleby H. (1989). The life and times of ded, text display editor. In Cognitive Ergonomics and Human-Computer Interaction. (Eds J.Long and A. Whitfield). Cambridge University Press: Cambridge.

Brannigan A. (1981). The Social Basis of Scientific Discoveries. Cambridge University Press: Cambridge.

Brown C.M.L. (1988). Human-Computer Interface Design Guidelines. Ablex: Norwood, N.J.

Brown J.R. and Cunningham S. (1989). Programming the User Interface: Principles and Examples. Wiley: London.

Burton F. and Carlen P. (1979). Official Discourse. Routledge: London.

Button G. (1990a). Going up a blind alley. In Computers and Conversation. (Eds P. Luff, N. Gilbert and D. Frohlich). Academic Press: London.

Button, G. (1990b). A clash of ideas: a response to Auer. Human Studies, 13, 4, 393-404

Callon M. (1987). Society in the making: the study of technology as a tool for sociological analysis. In The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology. (Eds W.E. Bijker., T.P. Hughes. and T. Pinch). MIT Press: Cambridge, Mass.

Card S.K., Moran T.P. and Newell A. (1983). The Psychology of Human-Computer Interaction. Lawrence Erlbaum Associates: Hillsdale, NJ.

Carroll J.M. (1990a). Towards an emulation-based design theory. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Carroll J. (1990b). Infinite detail and emulation in an ontologically minimized HCI. In Empowering People: CHI'90 Conference Proceedings. (Eds J.C. Chew and J. Whiteside). ACM Press: New York.

Carroll J. and Campbell R. (1989). Artifacts as psychological theories: the case of human-computer interaction. Behaviour and Information Technology, 8, 4, 247-256.

Carroll J.M. and Mack R.L. (1984). Learning to use a word processor: by doing, by thinking and by knowing. In Human Factors in Computers. (Eds J.C. Thomas and M. Schneider). Ablex: Norwood.

Carroll J.M., Mack R.L. and Kellogg W.A. (1988). Interface metaphors and user interface design. In Handbook of Human-Computer Interaction. (Ed M. Helander). Elsevier North-Holland: Amsterdam.

Cawsey A. (1990). A computational model of explanatory discourse. In Computers and Conversation. (Eds P. Luff, N. Gilbert and D. Frohlich). Academic Press: London.

Collins H.M. (1981). Stages in the empirical programme of relativism. Social Studies of Science, 11, 1, 3-10.

Collins H.M. (1989). Computers and the sociology of scientific knowledge. Social Studies of Science, 19, 4, 613-624)

Collins H.M. and Yearley S. (1990). Epistemological chicken. Unpublished paper read at the Discourse Analysis and Reflexivity Workshop, 31st March-1st April 1990, Loughborough University.

Coulter J. (1983). Rethinking Cognitive Theory. Macmillan: London.

Davidson D. (1984). Inquiries into Truth and Interpretation. Clarendon: Oxford.

Derrida J. (1978). Writing and Difference. Routledge and Kegan Paul: London.

Derrida J. (1982). Margins of Philosophy. Harvester: Brighton.

Dertouzos M.L. (1990). Redefining tomorrow's user interface. In Empowering People: CHI '90. (Eds J.C. Chew and J. Whiteside). ACM: New York.

Diaper D. (1989). The discipline of HCI. Interacting With Computers, 1,1, 3-5.

Dijkstra E. (1989). On the cruelty of really teaching computer science. Communications of the ACM, 32, 12, 1398-1404.

Downes S. (1987). Human-computer interaction: a critical synthesis. Social Epistemology, 1, 1, 27-36.

Draper S.W. and Norman D.A. (1986). Introduction. In User Centered System Design. (Eds D.A. Norman and S.W. Draper). Lawrence Erlbaum Associates: Hillsdale, New Jersey; London.

Drew P. (1990). Conversation analysis: who needs it? Text, 10, 1/2, 27-35.

Dreyfus H. and Rabinow P. (1982). Michel Foucault: Beyond Structuralism and Hermeneutics. Harvester, Brighton.

Eason K. (1989). Designing systems to match organisational reality. In People and Computers V. (Eds A. Sutcliffe and L Macaulay). Cambridge University Press: Cambridge.

Edmonds E. (1989). Judging software design. In People and Computers V. (Eds A. Sutcliffe and L Macaulay). Cambridge University Press: Cambridge.

Everest G. (1986). Database Management. McGraw-Hill: New York.

Finkelstein A. and Fuks H. (1990). Conversation analysis and specification. In Computers and Conversation. (Eds P. Luff, N. Gilbert and D. Frohlich). Academic Press: London.

Forrester M. and Reason D. (1990). HCI 'intraface model' for system design. Interacting With Computers, 2, 3, 279-296.

Foucault M. (1970). The Order of Things. Tavistock: London.

Foucault M. (1972). The Archaeology of Knowledge. Tavistock: London.

Foucault M. (1977). Discipline and Punish. Allen Lane: London.

Foucault M. (1979). The History of Sexuality. Vol.1. Allen Lane: London.

Foucault M. (1988). On power. In Politics, Philosophy, Culture: Interviews and Other Writings 1977-1984. (Ed L.W. Kritzman). Routledge: New York and London.

Frohlich D. (1988). Conversational dynamics for emergent explanations. In Proceedings of the 4th Alvey Workshop on Explanation. IEE: Stevenage.

Frohlich D. and Luff P. (1989). Conversational resources for situated action. In Proceedings of ACM CHI '89 Conference. ACM: New York.

Frohlich D. and Luff P. (1990). Applying the technology of conversation to the technology for conversation. In Computers and Conversation. (Eds P. Luff, N. Gilbert and D. Frohlich). Academic Press: London.

Fuller, S. (1988). Social Epistemology. Indiana University Press: Bloomington and Indianapolis.

Gaines B.R. (1984). From ergonomics to fifth generation: 30 years of human-computer interaction studies. In Human-Computer Interaction: Interact '84. (Ed B. Shackel). Elsevier North-Holland: Amsterdam.

Gardner H. (1985). The Mind's New Science. Basic Books: New York.

Garfinkel H. (1967). Studies in Ethnomethodology. Prentice Hall: Englewood Cliffs, New Jersey.

Garfinkel H. (1974). The origins of the term 'ethnomethodology'. In Ethnomethodology. (Ed R. Turner). Penguin: Harmondsworth.

Garfinkel H. and Sacks H. (1970). On formal structures of practical actions. In Theoretical Sociology. (Eds J. McKinney and E. Tiryakian). Appleton-Century-Crofts: New York.

Gentner D. and Stevens A.L. (eds). (1983). Mental Models. Lawrence Erlbaum Associates: Hillsdale, New Jersey.

Gilbert G.N. (1987). Cognitive and social models of the user. In Human-Computer Interaction: Interact '87. (H.-J. Bullinger and B. Shackel). Elsevier North-Holland: Amsterdam.

Gilbert G.N. and Mulkay M. (1984). Opening Pandora's Box: a Sociological Analysis of Scientist's Discourse. Cambridge University Press: Cambridge.

Gilbert N., Wooffitt R. and Fraser N. (1990). Organising computer talk. In Computers and Conversation. (Eds P. Luff, N. Gilbert and D. Frohlich). Academic Press: London.

Goffman E. (1981). Forms of Talk. Blackwell: Oxford.

Grudin J. (1989). The case against user interface consistency. Communications of the ACM, 32, 10, 1164-1173.

Grudin J. (1990). The computer reaches out: the historical continuity of interface design. In Empowering People: CHI '90. (Eds J.C. Chew and J. Whiteside). ACM: New York.

Hammersley M. and Atkinson P. (1983). Ethnography: Principles in Practice. Tavistock: London.

Harrison M.D. (1990). Modelling interactive behaviour from a sytem perspective. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Heritage J. (1984). Garfinkel and Ethnomethodology. Polity Press: Cambridge.

Hester S. (1981). Two tensions in ethnomethodology and conversation analysis. Sociology, 15, 108-116.

Hicks D. and Potter J. (1990). Sociology of scientific knowledge: a reflexive citation analysis or: science disciplines and disciplining science. Unpublished paper presented at the Discourse Analysis and Reflexivity Workshop, 31st March-1st April 1990, Loughborough University.

Hooper K. (1986). Architectural design: an analogy. In User Centered System Design. (Eds D.A. Norman and S.W. Draper). Lawrence Erlbaum Associates: Hillsdale, New Jersey; London.

Hutchins E. (1990). The technology of team navigation. In Intellectual Teanwork: Social and Technological Foundations of Co-operative Work. (Eds J. Galegher, R.E. Kraut and C. Edigo). Lawrence Erlbaum Associates: Hillsdale, New Jersey; London.

Hutchins E. and Klausen T. (1991). Distributed cognition in an airline cockpit. In Cognition and Communication at Work. (Eds D. Middleton and Y. Engestrom). Sage: London.

Ince D. (1988). An Introduction to Discrete Mathematics and Formal System Specification. Oxford University Press: Oxford.

Joerges B. (1990). Images of technology in sociology: computer as butterfly and bat. Technology and Culture, 31, 2, 203-227).

Johnson-Laird P.N. (1983). Mental Models: Towards a Cognitive Science of Language, Inference, and Consciousness. Harvard University Press: Cambridge, Mass.

Jones M. (1990). Macthusiasm: social aspects of microcomputer use. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Kammersgaard J. (1988). Four different perspectives on human-computer interaction. International Journal of Man-Machine Studies, 28, 343-362.

Keith W. (1990). Resonse to Slezak: *Nein ich verstehe nicht*. Social Epistemology, 4, 4, 361-367.

Kellogg W.A. (1990). Qualitative artifact analysis. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Kellner D. (1989). Jean Baudrillard: from Marxism to Postmodernism and Beyond. Polity Press: Cambridge.

Kieras D. and Polson P.G. (1985). An approach to the formal analysis of user complexity. International Journal of Man-Machine Studies, 22, 365-394

Klein J. (1990). Across the boundaries. Social Epistemology, 4, 3, 267-280.

Landauer T.K. (1988). Relations between cognitive psychology and computer system design. In Interfacing Thought. (Ed J.M. Carroll). MIT Press: Cambridge, Mass.

Latour B. (1986). Visualization and cognition: thinking with eyes and hands. Knowledge and Society: Studies in the Sociology of Culture Past and Present, 6, 1-40.

Latour B. (1987). Science in Action: How to Follow Scientists and Engineers Through Society. Open University Press: Milton Keynes.

Latour B. (1988a). Mixing humans and non-humans together: the sociology of a door-closer. Social Problems, 35, 3, 298-310.

Latour B. (1988b). The politics of explanation. Knowledge and Reflexivity. (Ed S. Woolgar). Sage: London.

Latour B. (1990). Postmodern? No, simply amodern! Steps towards an anthropology of science. Studies in the History and Philosophy of Science, 21, 1, 145-171.

Lave J. (1988). Cognition in Practice. Cambridge University Press: Cambridge.

Law J. and Lynch M. (1988). Lists, field guides and the descriptive organization of seeing: birdwatching as an exemplary observational activity. Human Studies, 11, 271-303.

Lemert C. (1979). De-centered analysis: ethnomethodology and structuralism. Theory and Society, 7, 289-306.

Leps M.-C. (1990). Crossdisciplinary inquiry in the information age. Social Epistemology, 4, 3, 281-291.

Lim K.Y., Long J.B. and Silcock N. (1990). Integrating human factors with structured analysis and design methods: an enhanced conception of the extended Jackson system development method. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Livingston E. (1987). Making Sense of Ethnomethodology. Routledge: London.

Long, J. (1989) Cognitive ergonomics and human-computer interaction: an introduction. In Cognitive Ergonomics and Human-Computer Interaction. (Eds J.Long and A. Whitefield). Cambridge University Press: Cambridge.

Long J. (1990). The effectiveness of knowledge supporting HCI practice (or how to judge the acceptability of new approaches to theory in HCI). In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Long J. and Dowell J. (1989). Conceptions of the discipline of HCI: craft, applied science, and engineering. In People and Computers V. (Eds A. Sutcliffe and L Macaulay). Cambridge University Press: Cambridge.

Luff P., Gilbert N. and Frohlich D. (eds) (1990). Computers and Conversation. Academic Press: London.

Lynch M. (1985). Art and Artifact in Laboratory Science. Routledge and Kegan Paul: London.

Lynch M. (1986). Review of Heritage (1984). The Sociological Review, 34, 1, 203-206.

Lynch M., Livingston E. and Garfinkel H. (1983). Temporal order in laboratory work. In Science Observed. (Eds K. Knorr-Cetina and M. Mulkay). Sage: London.

Lyotard J.F. (1984). The Postmodern Condition: a Report on Knowledge. Manchester University Press: Manchester.

MacLean A., Bellotti V. and Young R. (1990). What rationale is there in design? In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Malone T.W. (1983). How do people organize their desks? Implications for the design of office information systems. ACM Transactions on Office Systems, 1, 1, 99-112.

Martin J. (1967). The Design of Real-Time Computer Systems. Prentice Hall: Englewood Cliffs, NJ.

Martin J. (1973). Design of Man-Computer Dialogues. Prentice Hall: Englewood Cliffs, NJ.

Mayer R.E. (1988) From novice to expert. In Handbook of Human-Computer Interaction. (Ed M. Helander). Elsevier North-Holland: Amsterdam.

McHoul A.W. (1982). Telling How Texts Talk. Routledge: London.

McHoul A.W. (1984). Being seen to read the signs: some extensions of the work of Sharrock and Anderson. Information Design Journal, 4, 1, 69-76.

McHoul A.W. (1986). The getting of sexuality: Foucault, Garfinkel and the analysis of sexual discourse. Theory, Culture and Society, 3, 2, 65-79.

McIlvenny P. (1990). Communicative action and computers: re-embodying conversation analysis. In Computers and Conversation. (Eds P. Luff, N. Gilbert and D. Frohlich). Academic Press: London.

McTear M.F. (1985). Breakdown and repair in naturally occurring conversation and human-computer dialogue. In Social Action and Artificial Intelligence. (Eds G.N. Gilbert and C. Heath). Gower: Aldershot.

Messer-Davidow E. and Shumway D. (1990). Introduction to symposium on crossdisciplinarity. Social Epistemology, 4, 3, 261-266.

Miller R.B. (1971). Human ease of use criteria and their trade-offs. IBM Report TR 00.2185. IBM Corporation: Poughkeepsie, New York.

Mills C.W. (1940). Situated actions and vocabularies of motive. American Sociological Review, 5, December, 904-913.

Mulkay M. (1979). Knowledge and utility: implications for the sociology of knowledge. Social Studies of Science, 9, 63-80.

Mulkay M. and Gilbert N. (1982). Joking apart: some recommendations concerning the analysis of scientific culture. Social Studies of Science, 12, 4, 586-613.

Mulkay M., Potter J. and Yearley S. (1983). Why an analysis of scientific discourse is needed. In Science Observed. (Eds K. Knorr-Cetina and M. Mulkay). Sage: London.

Newell A. and Simon H.A. (1972). Human Problem Solving. Prentice Hall: Englewood Cliffs, New Jersey.

Norman D. (1986). Cognitive engineering. In User Centered System Design. (Eds D.A. Norman and S.W. Draper). Lawrence Erlbaum Associates: Hillsdale, New Jersey; London.

Norman D.A. (1988). The Psychology of Everyday Things. Basic Books: New York.

Norman D.A. and Draper S.W. (eds) (1986). User Centered System Design. Lawrence Erlbaum Associates: Hillsdale, New Jersey; London.

Norman M. and Thomas P. (1990). The very idea: informing HCI design from conversation analysis. In Computers and Conversation. (Eds P. Luff, N. Gilbert and D. Frohlich). Academic Press: London.

Payne S.J. (1990). Looking HCI in the I. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Payne S.J. and Green T.R.G. (1986). Task-action grammar: a model of the mental representation of task languages. Human-Computer Interaction, 2, 93-133.

Pinch T.J. and Bijker W.E. (1984). The social construction of facts and artefacts: or how the sociology of science and the sociology of technology might benefit each other. Social Studies of Science, 14, 3, 399-441.

Poster M. (1990). The Mode of Information: Poststructuralism and Social Context. Polity Press: Cambridge.

Potter J. (1988). What is reflexive about discourse analysis? The case of reading readings. In Knowledge and Reflexivity. (Ed S. Woolgar). Sage: London.

Potter J. and Wetherell M. (1987). Discourse and Social Psychology: Beyond Attitudes and Behaviour. Sage: London.

Raudakowski P. (1990). Repair work in human-computer interaction: a conversation analytic perspective. In Computers and Conversation. (Eds P. Luff, N. Gilbert and D. Frohlich). Academic Press: London.

Robinson H. (1990). Towards a sociology of human computer interaction: a software engineer's perspective. In Computers and Conversation. (Eds P. Luff, N. Gilbert and D. Frohlich). Academic Press: London.

Rorty R. (1980). Philosophy and the Mirror of Nature. Blackwell: Oxford.

Sacks H. (1984). On doing 'being ordinary'. In Structures of Social Action. (Eds J.M. Atkinson and J.M. Heritage). Cambridge University Press, Cambridge.

Sacks H. (1987). 'You want to find out if anybody really does care'. In Talk and Social Organization. (Eds G.Button and J.R.E. Lee). Multilingual Matters: Clevedon.

Sacks H., Schegloff E. and Jefferson G. (1978). A simplest systematics for the organization of turn-taking in conversation. In Studies in the Organization of Conversational Interaction. (Ed J. Schenkein). Academic Press: New York.

de Saussure, F. (1978). Course in General Linguistics. Fontana: London.

Schegloff E.A. (1989). Harvey Sacks: an introduction/ memoir. Human Studies, 12, 3-4, 185-209.

Sebillotte S. (1988). Hierarchical planning as a method for task analysis: the example of office task analysis. Behaviour and Information Technology, 7, 3, 275-293.

Shackel B. (1959). Ergonomics for a computer. Design, 120, 36-9.

Shackel B. (1990). Human factors and usability. In Human Computer Interaction: Selected Readings. (Eds J. Preece and L. Keller). Prentice Hall: Hemel Hempstead.

Shackel B., Eason K., Gardener A. and McKenzie J. (1988). Human Factors Guidelines for the Design of Computer-Based Systems. HMSO: London.

Sharrock W. (1989). Ethnomethodology. British Journal of Sociology, 40, 4, 657-677.

Sharrock W. and Anderson B. (1986). The Ethnomethodologists. Ellis Horwood: Chichester: / Tavistock: London.

Sharrock W. and Anderson B. (1987). Epilogue: the definition of alternatives: some sources of confusion in interdisciplinary discussion. In Talk and Social Organisation. (Eds G. Button and J. Lee). Multilingual Matters: Clevedon and Philadelphia.

Sharrock W.W. and Anderson D.C. (1979). Directional hospital signs as sociological data. Information Design Journal, 1, 2, 81-94.

Shneiderman B. (1983). Direct manipulation: a step beyond programming languages. IEE Computer, 16, 8, 57-69.

Shneiderman B. (1987). Designing the User Interface. Addison Wesley: USA.

Silverman D. (1975). Accounts of organizations. In Processing People. (Ed T. McKinlay). Hill, Rinehart and Winston: New York.

Silverman D. (1987). Communication and Medical Practice. Sage: London.

Silverman D. and Torode B. (1980). The Material Word. Routledge: London.

Simon H. (1981). The Sciences of the Artificial. MIT Press: Cambridge, Mass.

Smith D.C., Irby C., Kimball R., Verplank B. and Harslem E. (1982). Designing the Star user interface. Byte, April, 242-282.

de Souza F. and Bevan N. (1990). The use of guidelines in menu interface design: evaluation of a draft standard. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Stewart T. (1990). SIOIS - standard interfaces or interface standards. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Storrs G. (1989). Towards a theory of HCI. Behaviour and Information Technology, 8, 5, 323-334.

Suchman L. (1987). Plans and Situated Actions. Cambridge University Press: Cambridge.

Sudnow D. (1978). Ways of the Hand. Routledge: London.

Sutcliffe A. (1989). Task analysis, systems analysis and design: symbiosis or synthesis? Interacting With Computers, 1, 1, 6-12.

Sutcliffe A. and Macaulay L. (1989). Editorial. In People and Computers V. (Eds A. Sutcliffe and L Macaulay). Cambridge University Press: Cambridge.

Turkle S. (1984). The Second Self: Computers and the Human Spirit. Granada: London.

Van Cott H. (1985). High technology and human needs. Ergonomics, 28, 8, 1135-1142.

Walker T. (1988). Whose discourse? In Knowledge and Reflexivity. (Ed S. Woolgar). Sage: London.

Weinberg G. (1971). The Psychology of Computer Programming. Van Nostrand Reinhold: New York.

Whiteside J., Bennett J. and Holtzblatt K. (1988). Usability engineering: our experience and evolution. In Handbook of Human-Computer Interaction. (Ed M. Helander). Elsevier North-Holland: Amsterdam.

Whiteside J. and Wixon D. (1987). Improving human-computer interaction: a quest for cognitive science. In Interfacing Thought. (Ed J. Carroll). Bradford/MIT Press: Cambridge, MA.

Winder R. (1991). Review of Brown and Cunningham (1989). The Computer Bulletin, 3, 3, 28.

Winograd T. (1989). Response to Dijkstra (1989). Communications of the ACM, 32, 12, 1412-1413.

Winograd T. (1990). What we can teach about human-computer interaction. In Empowering People: CHI '90. (Eds J.C. Chew and J. Whiteside). ACM: New York.

Winograd T. and Flores F. (1986). Understanding Computers and Cognition. Ablex: New Jersey.

Wittgenstein L. (1958). Philosophical Investigations. Oxford: Blackwell.

Wooffitt R. (1990). On the analysis of interaction. In Computers and Conversation. (Eds P. Luff, N. Gilbert and D. Frohlich). Academic Press: London.

Woolgar S. (1976). Writing an intellectual history of scientific development: the use of discovery accounts. Social Studies of Science, 6, 395-422.

Woolgar S. (1981). Discovery: logic and sequence in a scientific text. In The Social process of Scientific Investigation. Sociology of the Sciences Vol IV. (Eds K. D. Knorr, R. Krohn and R. Whitley). Reidel: Dordrecht.

Woolgar S.W. (1981). Science and ethnomethodology: a prefatory statement. International Society for the Sociology of Knowledge Newsletter, 7, 1/2, 10-15.

Woolgar S. (1983). Irony in the social study of science. In Science Observed. (Eds K. Knorr-Cetina and M. Mulkay). Sage: London.

Woolgar S.W. (1985). Why not a sociology of machines? The case of sociology and artificial intelligence. Sociology, 19, 4, 557-572.

Woolgar S. (1987). Reconstructing man and machine: a note on recent sociological critiques of cognitivism. In The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology. (Eds W.E. Bijker., T.P. Hughes. and T. Pinch). MIT Press: Cambridge, Mass.

Woolgar S. (1988a). Science: the Very Idea. Ellis Horwood: Chichester: / Tavistock: London.

Woolgar S. (ed) (1988b). Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge. Sage: London.

Woolgar S.W.(1989). Representation, cognition and self: what hope for an integration of psychology and sociology? In The Cognitive Turn. (Eds S. Fuller, M. de Mey, T. Shinn and S. Woolgar). Kluwer: Dordrecht.

Woolgar S.W. (1990). Configuring the user: a look at usability trials.Unpublished paper presented at the Discourse Analysis and Reflexivity Workshop, 25th-26th September 1990, Lancaster University.

Wynne A. (1986). Reading and writing: sociology. Unpublished paper presented at the Discourse Analysis and Reflexivity Workshop, April 1986, York University.

Young R.M. (1990). Evaluating cognitive simulation models in HCI. In Human-Computer Interaction: Interact '90. (Eds D. Diaper, D. Gilmore, G. Cockton and B. Shackel). Elsevier North-Holland: Amsterdam.

Zimmerman D. and Pollner M. (1971). The everyday world as a phenomenon. In Understanding Everyday Life. Routledge and Kegan Paul: London.